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THE DUBLIN

# QUARTERLY JOURNAL

OF

# MEDICAL SCIENCE.

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## PART I.

## ORIGINAL COMMUNICATIONS.

ART. I.—*On Scalds of the Larynx.* By PHILIP BEVAN, M.D., Fellow and Professor of the Royal College of Surgeons, Ireland; Surgeon to Mercer's Hospital, &c.

WILLIAM CARROLL, aged two and a half years, a healthy child, from Adam-court, was brought to Mercer's Hospital, at 7 o'clock, P. M., on the 25th of September, 1852, ten minutes after having drank boiled flax-seed tea from the spout of a kettle. He screamed violently after the accident, but did not suffer from difficulty of breathing, and, therefore, the parents would not leave him in hospital.

On the following morning, Sunday, he was again brought to hospital: his parents stated that he suffered very little till 10 o'clock last night, when his breathing became difficult. At 10 A. M., i. e. fifteen hours after the accident, his respiration was most difficult, hurried, and stridulous; pulse rapid and small; no mucous rhonchus; no crepitus; face very slightly congested; not comatose. With great difficulty we made him swallow medicine, as his mouth and fauces were sore; the epiglottis is swollen and hard, the size of a nut. He was ordered a mixture of two grains of tartar emetic in an ounce each of water and syrup, a teaspoonful to be taken every ten mi-

nutes till vomiting supervened; after a few doses he vomited freely; he was ordered a grain of calomel every hour, and three leeches to be applied over the sternum. At 2 o'clock P. M., symptoms much worse, his breathing difficult and croupy; he falls into a slumber every minute; his eyes remain half closed; no other change; ordered three leeches to the sternum; continue the calomel.

4 o'clock P. M. A consultation was held, to decide as to the immediate performance of tracheotomy. The leeches had decidedly relieved the respiration; when we entered the ward he was dozing, but easily awakened; there was not so much croupy respiration, although there was a considerable amount of mucous rhonchus over a great part of the chest. Pulse about 150, not weaker than before the last leeching; the bowels had been purged once. After careful consideration, my colleagues and I agreed to delay the operation, as the child had been so much relieved by the leeching, and the pulse remained sufficiently strong to bear a repetition of the same treatment. He was ordered two grains of calomel, and three of compound chalk powder every hour, and to have three leeches again applied over the sternum; also, mercurial ointment over the axilla and abdomen. 7 o'clock P. M. Child lies half comatose, but can be roused; pulse about 120; respiration 30 in the minute; when roused, he sits up, takes his powder, and falls asleep again; skin warm; respiration less croupy.

27th. To my surprise, I found him sitting up in bed; the croupy respiration greatly relieved; lungs much improved; no pneumonic crepitus; bronchitic rales less distinct, but a good deal of mucus still rattling in the large bronchial tubes; he coughs freely and expectorates the mucus easily; pulse 120; he has had three large green stools during the night; skin cool. Continue the calomel every third hour; milk diet.

28th. Calomel stopped, as the bowels were several times purged.

He left the hospital on the 30th, quite well, except slight cough. The epiglottis, although reduced in size, still feels thicker and fuller than natural.

Patrick Byrne, aged three years, was admitted into hospital on the 7th September, 1858, having swallowed boiling water from the spout of a kettle at 6 o'clock, P. M., on the evening of the 6th, i. e. fifteen hours previously. He lies dozing in his mother's arms; respiration difficult and croupy; face congested; his mouth, as far as I can see, is white; and the epiglottis is hard, round, and like a large cherry: this proves that

either the vapour or the water had come in contact with the epiglottis. The mother stated that he spat out the water as soon as he took it into his mouth. Ordered: three leeches to the sternal region; solution of tartrate of antimony and potash, one grain to the ounce of water, two teaspoonfuls every ten minutes till vomiting be produced. A cathartic turpentine enema, if bowels not acted on by the tartrate of antimony; one grain of calomel every hour, and mercurial ointment into each axilla. At 3 o'clock P. M., child still drowsy; face turgid; pupils contracted; inspiration very stridulous and difficult, mucous rattling in large bronchial tubes, and sonorous râles over base of both lungs; leeches bled freely; bowels not opened by enema; is very thirsty, constantly craving cold water; repeat enema immediately; three leeches to the top of the sternum, followed by stuping with hot water. 9 o'clock, P. M., saw him again: bowels opened freely by enema; leeches seemed to give him great relief; his breathing less sonorous; still very drowsy; lungs as before. Calomel to be continued, unless hypercatharsis is induced.

8th. In the morning I found him sitting up in bed, much relieved, but breathing is still sonorous, and less mucus in the bronchial tubes; he is reported to have been very drowsy, but purging commenced at 3 o'clock in the morning (my notes do not state if the stools were green). Repeat calomel every second hour; he continued to improve during the day.

9th. Still improves; slept well, but is so cross and unruly that I cannot satisfactorily examine his chest; however, there is less mucus rattling in his lungs, which he coughs up with tolerable ease; the epiglottis is still swollen, but probably smaller than at first; bowels twice moved; his mouth is very sore from the mercury, so that he can only eat the softest panada. He continued to improve, and left the hospital in a few days, perfectly well, except slight cough.

Bridget M'Bride, one year and ten months old, a delicate and feeble child, from 44, Cuffe-street, admitted on the 1st of October; had taken a draught of boiling coffee out of a kettle, at about 12 o'clock noon. She was brought to Mercer's Hospital at 2 o'clock P. M., but was immediately taken home; she was brought back again at 3 o'clock P. M., i. e. three hours after the accident. I was sent for, and saw her soon after; she was lying with her mouth open, half asleep; her breathing stridulous and croupy. My finger introduced into the mouth detected the epiglottis very hard and round, of the size of a gooseberry; mouth and lip scalded; feet cold; face and chest

bathed in cold sweat. I never saw a case so bad in so short a time after the accident. Ordered two leeches to the upper part of the sternum; an emetic of two grains of tartar emetic to an ounce of water, a teaspoonful every ten minutes till vomiting was produced; if bowels not purged, a simple enema; one drachm of mercurial ointment in each axilla, and one grain of calomel every hour. 7 o'clock P. M. Appears worse; more difficulty of respiration; pulse most rapid and weak; lies, as before, comatose; stridulous and sonorous râles over the back of the lungs, but no dulness on percussion; alæ nasi forcibly dilated; deep pits, during each inspiration, above the clavicle, from the violent efforts made to inflate the lungs; eyes suffused, and pupils dilated. The emetic vomited her well, but, as the bowels were not moved, the enema was administered, and produced a large evacuation; the leeches bled freely, and seemed for some time to give her relief. Although the pulse was weak, I ordered two leeches to the sternum, and stuping afterwards, and mercurial ointment over the back of the chest. I confess I did not expect to see her alive in the morning. If sufficiently strong, I directed the resident pupil, Mr. Cumming, to apply leeches again during the night, and to continue the calomel.

October 2nd, twenty-one hours after the accident. I was informed that she was better: I found her asleep; her breathing very croupy and difficult; the pits above the clavicles seemed as well marked as before, but, as soon as she roused up, all the symptoms seemed better, the breathing less noisy; still the improvement, although evident, was not considerable: sibilant and sonorous râles are heard over the most part of the chest, and a quantity of mucus is rattling in the bronchial tubes; pulse very rapid, not weaker than last night; she cried out lustily, and drank a little milk when awake. Two more leeches over the sternum, mercurial ointment over the chest; continue the calomel. 3 o'clock P. M. I thought her breathing more difficult; pupils dilated; very restless; two more leeches.

3rd. The child is much improved: breathing nearly natural and quiet, but extremely restless, and her mother says she is more sick than ever; however, her respiration and all other symptoms are decidedly improved; nurse reported that she had two green stools at 2 o'clock A. M, i. e., thirty-five hours after my first visit; during that time she had been constantly rubbed with mercurial ointment, and had taken thirty-six grains of calomel. I passed my finger over the epiglottis; it is still large and hard. 5 o'clock P. M. I found the child sleeping calmly, and respiring quite easily.

4th. Child breathes naturally, no croupy sound; mouth nearly well; green stools; coughs up mucus freely.

5th. Patient is gradually improving. Respiration natural, except slight bronchitis; no croupy inspiration; unless that she is cross, she seems well, but is very weak; epiglottis is still large, but certainly reduced in size. No treatment.

6th. Left the hospital quite well, except a scald of the lip and mouth.

Ann Jane Fitzgerald, aged two and a half years, a delicate but healthy child, living in Fishamble-street, admitted into Mercer's Hospital on the 30th September, 1859, at 3 o'clock in the morning. Her grandmother stated that at 7 o'clock on the previous evening she drank a gulp of boiling water from a kettle, but had spit it out instantly. She brought her immediately to an institution in the city, and was told that it would not signify. She gave her a small dose of castor-oil, and brought her home. She slept soundly, but her breathing gradually became difficult, and continued so till her arrival at the hospital. The resident pupil gave her some ipecacuanha, but it had not any effect; and I visited her at 4 o'clock A.M., eight hours after the accident. She laboured under all the usual symptoms. She lay on her back, semi-comatose, eyes half closed, mouth open, lips scalded, respiration stridulous, inspiration long and sonorous, expirations short and quick, larynx rapidly raised and depressed, hands cold, slightly clammy. She is tossing from side to side; pupils are contracted and fixed; pulse rapid, I could not count it; epiglottis the size of a marble, and very hard.

I ordered one grain of tartar-emetic in half an ounce of water, a teaspoonful every ten minutes till vomiting was produced; three leeches over the upper bone of the sternum; the leeches were applied before the vomiting commenced. A turpentine enema was administered at the same time, and mercurial ointment was rubbed into each axilla. I waited till I saw all the above remedies employed. The emetic acted after the third dose, but nothing except water and mucus escaped, as it was some hours since she had taken food. I ordered that, as soon as her stomach was settled, she should get two grains of calomel every half hour, and that, as soon as the leeches fell off, a poultice of linseed-meal should be applied over the bites, provided none of them were bleeding too freely.

9 o'clock A.M. I was informed that the bowels had been once freely opened by the enema, and that she seemed rather

relieved by the bleeding, but had gradually relapsed into her present state. She has taken eighteen grains of calomel since my last visit; she is now in nearly the same state as before, lies with closed eyes, pupils contracted, but, when roused to swallow, the pupils dilate. At every effort to swallow she is thrown into a sort of spasm, but at other times lies half comatose; her respiration is just as difficult, and all the symptoms fully as bad as before the treatment, and her pulse is excessively rapid and weak; her cheeks are pale, and lips slightly livid. As she had not improved, and her pulse was so weak, I certainly thought she was a case which would require operation, and therefore directed a consultation with my colleagues to be called, if the symptoms should not be improved by 12 o'clock at noon. In the meantime I ordered three more leeches to be applied, and calomel, two grains every half-hour, as before. At 12 o'clock I found her sitting up in bed looking about her; breathing still hoarse, but not so sonorous as before. Her relations think her decidedly better. She has had two light-green stools, but the colour was less deep than usual. I therefore continued the calomel every hour, in one-grain doses; the improvement must have been very sudden, as the resident pupil reported that she was as bad as ever half an hour before my visit. At half-past 3 o'clock P.M. she had one stool, also free, but she lies still apparently asleep; breathing sonorous, but not so loud as before; respiration 52 in the minute; pulse 130; she has been slightly bleeding into the poultice, which I now removed; I did not think her so much improved as I expected. At 10 o'clock P.M. I received a note from Mr. O'Connor, the resident pupil (who watched the case for me with the greatest attention throughout), stating that she had three green stools of large size and deep colour; that the respirations were 48 in the minute, and pulse strong; breathing very much improved; and she had been crying for food. Calomel continued every second hour.

October 1st. The child did not sleep much during the night, but continued to breathe quietly, about 46 in the minute. No purging since last night; she seems sleepy, but not at all comatose. Dr. Jameson and myself felt the epiglottis, which is still round and hard as a marble; some saliva is constantly trickling from her mouth, sufficient to wet a small pocket-handkerchief, but her gums are neither red nor ulcerated, nor are they painful to the touch; a good deal of mucus is rattling in the larger bronchial tubes. Stop all treatment.

2nd. Pulse 120; respiration easy; no sonorous inspiration;

slight catarrh; some mucus still in the bronchi; wishes for food; one large green stool; no ulceration or redness of the gums, but saliva still trickles from the mouth.

4th. Child dismissed, quite well, except a very slight cough.

In this case the child took fifty-six grains of calomel, forty-four in eleven hours, before the green stools made their appearance. She visited the hospital on the 8th, perfectly well.

Of the many accidents to which children are liable, one of the most painful and fatal is that produced by swallowing or attempting to swallow boiling fluids. Having seen a considerable number of such cases, and being little satisfied with the results of the treatment adopted, I have ventured on a revival, or, rather, a modification of the old in preference to what may be called the approved method, i.e. tracheotomy.

The symptoms produced by this accident are so well-known, that I will not enter on them in detail, but merely allude to them, for the purpose of dividing them into three stages. In the first stage, the mouth and fauces alone are affected, but the respiration is unimpaired. In the second, the ingress of air is impeded by laryngitis; and œdema glottidis, and incipient congestion of the lungs, are the result. In the third stage, engorgement of the lungs, and consequent congestion of the brain, are added to the previous symptoms. This division can be made in all cases; in some, no doubt, the second follows the first stage so rapidly that they might be said to be simultaneous; but, in the majority, an interval of several hours takes place between them. The child, immediately after the accident, suffers most intensely; he keeps his hand to his mouth and larynx, screams violently, and cannot swallow. After the fright and immediate pain have passed over, no bad symptom may come on for several hours; during this period he may play or sleep, or even eat fruit or cakes, given to him by his parents to stop his cries. At this time the mouth and fauces are red; white blisters are seen on the lips, cheeks, and near the root of the tongue, but he breathes without difficulty. That the symptoms are so trifling at first, should be carefully borne in mind by the surgeon. I have known several cases where children lost their lives from inattention to them in this stage; and the mistake is the more liable to occur, as frequently the symptoms are in no way more severe or different than those arising from simple scalds of the mouth, which will, of course, be rapidly cured without any treatment.

The second stage, whether it comes on rapidly or slowly, presents much more alarming symptoms. The breathing is

stridulous and croupy, rapid and much embarrassed; the features are bloated and pallid; pulse rapid; skin cold and damp; he has a tendency to doze, but can readily be roused In addition to the changes observed in the mouth during the first stage, on examination, the epiglottis will be felt by the finger to be round and hard, of the size and shape of a gooseberry or large nut, whilst sonorous and sibilant râles will be heard over the greater part of the chest. These symptoms continue for several hours, and are gradually changed into those of the third stage; in it, the respiration becomes more and more laboured and croupy; the efforts to inspire are more painful; the larynx is drawn up and down rapidly, and deep fossæ are formed above the clavicles during the convulsive efforts to fill the chest. The little patient lies with his head thrown back; his eyes fixed, half open, and turned up under the lids; the pupils are fixed and dilated; his face is swollen and purplish; his mouth half open; he tosses his arms about; and, although semi-comatose, vainly seeks to relieve the sense of impending suffocation from which he is suffering. During this stage, the mucus is heard rattling up and down in the lungs and bronchial tubes; coma gradually increases, until he either dies in that state, or is carried off in a paroxysm of convulsion.

The treatment recommended and adopted by the most distinguished surgeons and physicians of the present day may be summed up in a few words: at first, antiphlogistics; and then, the operation of tracheotomy; the only difference of opinion being as to the time at which the operation should be performed. Some prefer to operate in the early stage: Dr. Copeland, in his excellent Dictionary, says: "An early recourse to the operation is particularly indicated, when laryngitis has been caused by swallowing acrid, corrosive, or boiling fluids, as the means of cure recommended do not act so rapidly, and an early opening into the trachea facilitates the treatment of the injured parts." Dr. Watson also advocates the early recourse to operation; he seems very sanguine of its success, but his experience seems to have been derived rather from tracheotomy for laryngitis in the adult than in that of the infant. He says: "If an artificial opening be made while the patient's strength is still entire, and before his system is poisoned by the venous blood, or his lungs are overwhelmed with sanguine congestion and serous effusion, it will almost infallibly save his life." He therefore says that "it is a bad and foolish practice to wait."

Others defer the operation: thus Mr. Porter says: "When we have such abundant evidence of the occasional success of antiphlogistic measures, I think they should be adopted and

persevered in until the breathing becomes so affected that there is every reasonable probability of the operation becoming necessary. At this crisis it should not only be proposed, but its advantages impressed upon the patient's friends. Although a person might now and then be subjected to it without absolute necessity, yet I feel convinced that numbers would be preserved." Also Dr. Jameson[a] (who published thirteen most interesting cases of tracheotomy) says: "Should the usual remedies, such as emetics, leeches, heat to the surface, fail to allay the urgent symptoms; but when the breathing becomes stridulous and croupy, or amounting to a mere pant from spasm of the glottis, the pulse quick and small, the temperature of the body diminished, the head drawn back, face congested, eyes half open, inclination to coma and difficult deglutition, I should, on the first accession of these symptoms, at once be inclined to operate." So also Erichsen says[b]: "If urgent symptoms of dyspnœa have set in, tracheotomy must be performed without delay." That the operation is by no means a successful one, is acknowledged even by those who recommend it. Erichsen says, "that in the majority of the cases that have fallen under his observation, in which this operation has been performed, the issue has been a fatal one from the speedy supervention of broncho-pneumonia." No doubt, a few successful cases are to be found in the periodicals, but these are of little assistance in our endeavour to arrive at the value of the operation, as the many unsuccessful ones are never reported. Dr. Jameson recorded three cures out of eleven operated on; and, from my own experience, I would consider this a fair average. The want of success has been attributed by some surgeons to the hemorrhage during the operation; but this does not accord with my experience, as in only two of the cases recorded by Dr. Jameson was there any hemorrhage; and in none of the fatal operations I have seen was there bleeding to any extent. Other surgeons attribute the deaths to the delay of the operation; but in several cases I have seen the operation performed eight hours after the accident, and still the result was death. It is far more probable that the broncho-pneumonia and infiltration of the lungs are the real causes of the fatality. As in drowning, or other cases of asphyxia, this takes place sometimes in a very short time after the accident, and will not be improved by the operation; indeed, it is a question of much difficulty to decide whether the admission of cold air, directly into the trachea and bronchial tubes, may not accelerate the

[a] Dublin Quarterly Journal of Medical Science, No. IX. [b] Surgery, page 311.

congestion and inflammation of lungs already diseased; for this reason it is that, although, as soon a free opening is made into the trachea, the patient always appears much improved, so much, indeed, as frequently to mislead both the surgeon and the patient's friends; still, after a few hours, all the symptoms recur, and increase till he dies, just as if the operation had never been performed. In fact, the asphyxia is cured, but the infiltration and congestion of the lungs continue, and kill the patient.

The cases I now publish were fully so bad so to justify the operation; the stridulous breathing; bloated, pale features; fixed pupils; rapid, feeble pulse; congested lungs; cold surface; hard, erect epiglottis; and incipient coma, were certainly as bad as in many cases where I have both operated myself and seen the operation performed by other surgeons without success. I therefore have a right to conclude that, had the operation been performed, not more than one out of the four cases would have survived.

If a patient is *in extremis*, then, no doubt, the surgeon is justified in trying the operation, as, although nearly hopeless, it is the only treatment which can save the patient from immediate dissolution; and that occasionally such a case may recover, is proved by a few instances reported in the periodicals and in books. Still I believe that the antiphlogistic treatment, if conducted with sufficient rapidity, will be far more successful; but, to be successful, it must be rapid and energetic in the extreme; a few hours are all we have for treatment, unless we can suddenly stop, or at least delay the progress of the second stage; the injury to the lungs and brain will be such as to preclude the possibility of success.

I commence the treatment with an emetic, followed by a cathartic enema, at the same time a few leeches, according to the strength of the child, are applied over the upper margin of the sternum, the bleeding being encouraged by a warm fomentation or poultice. Should the child sleep, I leave directions that I shall have notice of his having awakened, or visit again in a few hours. Should symptoms of the second stage appear, I commence calomel in doses according to the age and strength of the child and the severity of the symptoms. The leeches should be repeated every third or fourth hour, if the strength of the child permits it, great care being taken to avoid hemorrhage from the bites, or exhaustion from loss of blood; for this reason it is indispensably necessary that either the surgeon or a competent assistant should visit before each repetition of the leeches. The calomel should be repeated at very

short intervals, every hour or half-hour, and the body should be rubbed with mercurial ointment, or a portion of the ointment be placed in each axilla, the object being to produce the specific effect of the mineral in the shortest possible time

The effect of this line of treatment has been most satisfactory. The leeching gave relief for the moment, and, although its effects were transitory, yet it gave time for the action of the mercury, in all probability promoted its absorption, and at the same time diminished the congestion of the brain. As soon as the mercury produced the green stools, the symptoms in every case were improved, and the child recovered. The lungs were the first organs relieved, the brain next, and the larynx last of all; indeed, for several days, after all other bad symptoms had disappeared, the epiglottis remained swollen and hard. Should the case have arrived at the second stage, the orthopnœa being considerable, and the congestion of the lungs rapidly coming on, I at one and the same time use the emetic, leeches, enemas, and frictions with mercurial ointment. As soon as the stomach is sufficiently settled, after the emesis, I give the calomel, in two-grain doses, and usually repeat it every half-hour, till its effects are produced, the leeches being repeated as before, every second or third hour, according to the strength of the patient. It may be said that similar treatment has been tried by others. No doubt, leeching and bleeding have been much practised. Dr. Watson seems to place his principal reliance on this and on tracheotomy; but he gives some cases himself where it did not succeed; and I have seen two where it was carried to such an extent as to produce complete exhaustion, and still the disease appeared to me to progress the more rapidly. In fact, I look upon its effects as merely transitory, and of importance in saving the brain and lungs from impending congestion until the mercury has time to act. Mercury has been lauded by some, and dispraised by others. Dr. Watson does not depend on it; he says: "We cannot reckon upon its influencing the system *in time*, nor upon any marked improvement of the symptoms, when it does produce its specific effects." No doubt, if given as it generally is, two grains of calomel every third hour, the patient will die before its effects are produced; but, if given every half-hour, in one or two-grain doses, its effects will be produced in an extremely short time, especially if assisted by mercurial inunction over a large extent of surface. This method of rapidly producing the effects of mercury on the system has been taught us by cases of injury of the head, where in a few hours we have salivated adults by small and rapidly repeated doses of the metal; in the present

cases the green stools were produced in one so early as eight hours after the first administration of the mercury, and, in the other cases, in periods varying from eighteen to twenty-six hours. I do not deny that patients may die, even after the action of the mercury on the system, but I have never seen an instance of it except one where I had previously performed tracheotomy, from which circumstance I now doubt whether he would not have recovered had the operation been omitted. It may be said that four cases are not sufficient to establish any line of practice. My answer is, that they are the only cases I have had since I made use of this mode of treatment, and as they were all successful, I think I am justified in publishing them, in hopes they may lead others to follow the same course, and publish the results. It might be thought that the administration of such large doses of calomel in so short a time would be objectionable, but, except in one case, where an increased flow of saliva for a couple of days, and, in another, where a slight diarrhœa succeeded, no bad effects whatever resulted, and the children appeared to enjoy perfect health after a few days.

Since the above was written, a valuable statistical report has appeared in the Number of the Medical Times and Gazette for October 22, 1859. From it we learn that the operation of tracheotomy for scalds of the larynx has been even more fatal in England than in Ireland, since only three cases recovered of fourteen operated on; and, as in one of these it seemed to the author doubtful whether the glottis was scalded at all, this would leave the proportion of recovery only one in six and a half. This report confirms many of my statements: First, that the fatality of the operation did not arise from delay, as in most of them the operation was performed in less than seven hours, and in one so early as one hour and a half after the accident. Secondly, that most of the cases seemed to be improved by the operation for a short time, although they afterwards died. Thirdly, that, in several at least, disease of the lungs, either bronchitis, pneumonia, or broncho-pneumonia, was the immediate cause of death. Unfortunately, the pathology of several cases is very imperfectly detailed; in only one case hemorrhage, and in three, exhaustion, was the cause of death.

Art. II.—*Medical Cases and Observations.* By Daniel Griffin, M.D., M.R.I.A., &c., Physician to the County of Limerick Infirmary, &c., &c.

### SINGULAR CASE OF HYSTERIA IN A BOY.

I do not know if the case I am about to describe deserves the name I have given it, but it is of that class in which a state of hyperæsthesia, or what would, I believe, more properly be called an unusual mobility of the nervous system, produces anomalous symptoms. A man called on me at eight or nine o'clock in the evening, requesting me to see a boy named James Shaughnessy, who was suffering from severe oppression and palpitation, with great debility. He left it optional with me to go either then or in the morning, but, from the manner in which he described the symptoms, I apprehended pericarditis, and told him valuable time might be lost if the visit was deferred. He then asked me to see him at once. I found a handsome, pale-looking boy, about thirteen years of age, lying on a bed, and breathing with such extreme rapidity that I had never seen anything like it before in any illness. His respiration was from 140 to 160 in a minute. On applying a stethoscope to his chest, there was a loud bellows-sound, so exactly resembling that which attends pericarditis, that I do not think the most practised ear could detect any difference between them. I at first wondered at its extent, for it was heard over the whole front of the chest, and expected to find it more intense and pronounced in the region of the heart, but to my surprise I found the heart beating soft and natural at 88, as was also the pulse. Its sounds were very distinct and clear, though difficult to be heard, from the loudness of the soufflet in the lungs. On further examination, there was not the least evidence of structural disease in either heart or lungs. The most singular portion of the phenomena, however, was, that whenever he fell asleep, the rapid breathing instantly ceased, and his respiration became quite normal, but it recurred again whenever he awoke, and went on with as much rapidity as ever. His skin was quite cool, his tongue yellow, and rather loaded, and his bowels confined. On inquiry I learned that he had received a severe shock, three or four weeks previously, by the death of his mother, to whom he was much attached, and who had died suddenly in her confinement ; that, being a weakly child, she had always paid him great attention, procuring him those delicacies and comforts which only a mother's thoughtful care can supply ; that after her death he had fallen into the hands of his grandmother, and was neglected ; and that his present illness had lasted three or four days. Another prac-

titioner had seen him a day or two before my visit. He ordered a mustard plaster to be applied to his chest, and prescribed an antispasmodic mixture. He overlooked, however, a most important point in his case, the state of the digestive organs, so these remedies produced no benefit. A brisk cathartic of calomel and black draught produced copious discharges of a dark colour, attended by extreme fœtor, from the bowels. This was followed by full doses of quina, three times a day, and nourishing food, and by attention to the digestive organs he was restored to perfect health in about a week, and has had no relapse since.

### FLOODING AFTER DELIVERY.

There is one class of cases of this kind on which I am anxious to make some observations, because I do not believe that the profession generally, at least the junior members of it, are at all aware to what an enormous extent they may be prevented. I mean those which arise from an over-hasty extraction of the placenta. The attempt to interfere too much with the processes of nature, and among them the over-anxious desire to get the placenta away speedily after the birth of the child, has been deprecated by many excellent writers, most of whom agree that unless hemorrhage occurs, or violent and irregular spasm, endangering the life of the woman, the mere retention of the placenta is not a sufficient cause for interference, unless an hour has passed. For many years I have made it a point to follow this excellent rule, and not to interfere with the placenta until it is thrown off naturally, unless this event is delayed beyond the time specified, or there is profuse flooding from its partial separation. It will usually come away within this time ; and I leave the matter entirely to nature, avoiding for a time any, even the most moderate, traction of the cord, and merely making pretty firm pressure over the uterus itself, not with a pad and bandage, but with the hand alone, which latter is infinitely preferable. The advantage I have derived from this practice is more than I could have believed possible. It is not that cases of flooding are rare, and fatalities from it still rarer, but both have been altogether excluded in every case in which I have been present at the labour, or in time for its conclusion. I wish to insist upon this fact, because, from casualties which I have heard of from time to time in the practice of others, as well as from the dangerous tendency sometimes of my own cases before I adopted this course, I am sure its importance is not at all sufficiently appreciated. That the result I speak of is not fortuitous, or a mere piece of good luck, is

shown by its constancy during many years, and by the fact that I have had my full share of cases of flooding from placenta prævia and other causes during the whole of this interval, but from this cause absolutely none. Another proof lies in the fact that in so far as I have on any occasion been tempted to deviate from this course, in so far exactly has the tendency to dangerous hemorrhage reappeared. I have had another advantage in the practice; so far from losing time, I am perfectly satisfied I have gained or saved a great deal of it on several occasions, where less attention to the circumstances I speak of would have kept me in attendance on my patient many hours longer. I do not mean to say that several members of the profession do not understand the advantages of this plan, and practice it, but, from the circumstance that many natural cases of labour will get well under great varieties of management, I am convinced that the generality will hardly bring themselves to believe that its importance is not exaggerated; and I think it probable the prevalence of this error leads not unfrequently to the accidents I speak of. The temptation, indeed, to complete the case is great. The woman has gone through her labour well, and without delay or accident. The infant also is well. The patient's friends, who understand very well that the labour is not completed on the mere birth of the child, are asking the medical attendant if "the after-birth has come ?" or "if the woman is yet safe ?" He is anxious to get out of the house, to other patients. He tells them, "not yet;" and while he sits apparently idly there, he seems to be, as it were, neglecting a duty. If he is impatient or inexperienced, this inactivity does not continue long. He pulls away at the cord; makes strong friction over the uterus; gets the woman to cough, or gives her a pinch of snuff to make her sneeze. After an interval, he pulls again more strongly, and either snaps the cord or brings away the placenta suddenly, before the uterus has had time to contract. In the one case he has got himself into the unpleasant dilemma of having to follow the placenta, and extract it with his hand; in the other, of having his hasty efforts followed by a troublesome and alarming hemorrhage, which may detain him two or three hours or more; in both, he finds in the end, but only at the cost of much anxiety and trouble, that in this, as in many other instances, the old adage is true, "the more haste, the worse speed."

But the matter may be much worse than this. It sometimes happens, that, being about to leave the house, the practitioner goes to see if his patient is all right before his departure. Having left her comfortably arranged a short

time previously, he finds her now with pallid face, short breathing, feeble and fluttering pulse, and tumid abdomen, yet there is no external hemorrhage. The uterus has swelled up, is filled with large clots, and filling still; he makes pressure upon it, and gives her a stimulant. There is a sudden dash of hemorhage, with enormous clots; in a minute or two, he feels the uterus, like a round, hard globe, in the cavity of the pelvis; the woman breathes more freely, and her pulse is less rapid, and firmer. The same thing is repeated three or four times, with a considerable additional loss each time. At length he has made her more comfortable, and thinks he has overcome the tendency finally. If, however, he disregards these ominous signs of an uncontractile uterus, and goes away, the chances are, that within an hour or more, an alarming hemorrhage sets in. Pressing messages are sent for him; he is not found, perhaps, and when he arrives at last, he finds his patient dying or dead. The case looked very well at first. The patient had a rapid labour; the placenta was got away rapidly; everything seemed to promise well, but it was a case in which too much was done. The first error was unperceived, and, unconsciously to him, she fell a victim in part to what has been well called "meddlesome midwifery," and in part to his subsequent desertion of her, at a time and in circumstances in which his presence, so far from being unnecessary, was of the last importance.

That these are not imaginary dangers will be fully felt by those who, from experience, are aware what a strong and almost uncontrollable tendency to hemorrhage there is in some females, and what an apparently inadequate cause will sometimes set it in operation. The duty of the practitioner to his patient in such circumstances is well set forth in a paper by the late Mr. Labatt, which has been published by his son, Mr. Hamilton Labatt, in the Dublin Quarterly Journal for May, 1858. He gives one instance there of a lady whose life was often in peril from this tendency, in which, with a praiseworthy devotion not likely to be often imitated, he remained by his patient's bedside, making pressure upon the uterus, in the manner I speak of, with the hand alone, for upwards of five hours, the result showing, in a safe and rapid recovery, the complete efficacy of this patient and persevering management. In this excellent and practical paper Mr. Labatt impresses very strongly and earnestly upon the junior practitioner the necessity of closely watching his hemorrhagic patient, even for some hours after all alarming symptoms have subsided, "having known," as he says, "several cases of sudden and unexpected sinking and

death long after all danger was supposed to have ceased." Of these he details the circumstances of a certain number of interesting ones which came within his own cognizance. He also combats the prevalent error, "that if, after the expulsion of the placenta, the uterus be felt well contracted, small, round, and firm over the pubes, there is no reason to apprehend hemorrhage;" and states that "though this may be true to a certain extent, yet, if due care be not taken to maintain this state of salutary contraction by the means already advised, the uterus may relax, and hemorrhage ensue." On both these points my own experience, though more limited, enables me very strongly to confirm all this eminent accoucheur has said, and to add the opinion which I have already endeavoured to enforce, that too early interference with the placenta is in many instances the cause of those after evils.

I wish to insist on the necessity of the practice I speak of for another reason besides those I have stated. I observe that in a late bold attempt to overturn one of nature's most ordinary and well-established laws, and to substitute for it one of nature's exceptions, it is laid down as a precept that the placenta must be extracted immediately after the birth of the child. I allude to the late attempt by Mr. Figg, in a clever paper published in the Medical Times and Gazette, to substitute the practice of turning in every instance for the ordinary mode of delivery by head presentation. While I offer the testimony of my experience as a strong protest against this precept, I must say, in passing, of the proposal in which it originated, that what astonishes me most in the discussions regarding it is the extreme toleration with which it has been received by the profession. That a practitioner should say that the operation of turning might be adopted with advantage more extensively than it is, and that many more cases were well adapted to it than those in which it was used, would be but natural; but that a man should seriously propose so entirely to revolutionize nature's ordinary proceedings as to substitute turning for head presentation in every instance, and that such a proposal should not be generally and immediately denounced as extravagant and dangerous, is to me very surprising indeed. In other departments of physical science it has been found invariably that whenever it is attempted to use the powers of nature for any purpose, in so far as this attempt is made in conformity with her ordinary rules or against them, in so far exactly is the attempt successful or unsuccessful; so that there cannot be a better test of the feasibility of any proposed proceeding than to consider it in reference to these rules, and observe if it run counter to them,

or agree with them. The same is the case in medical science also, with this important and serious addition, that the end aimed at being the preservation of human life, the failure in attaining this end necessarily involves the loss of it. In the case I speak of, the mode of delivery in natural labour, nature has pronounced so positively and decidedly as to her ordinary intention, that there cannot be a doubt on the subject. The same test exactly applies, and it needs neither a trial of the new mode on the part of the profession, nor the important admissions made by Mr. Figg himself as to the frequent occurrence of still births, or the occasional occurrence of a fractured arm, to pronounce that such a method is wrong, and ought not to be adopted. There can be no question that Mr. Figg is an expert manipulator, though an indifferent logician, and a somewhat unscrupulous, yet, certainly, a candid moralist; nevertheless, the practice does not seem, even in his hands, to be very successful; and it is fearful to contemplate what the consequences might be if it were adopted generally by the profession on his advice, and practised as it must needs often be, by persons of much less dexterity, and of very little experience. I trust that these remarks will not be considered out of place, as I feel that no one whose opinions coincide with those I have expressed ought to withhold them on so important a subject, under all the circumstances. The whole weight of my experience, too, is against that which has been deduced as a kind of corollary from the practice, viz., the immediate extraction of the placenta. Indeed, the physiology of the subject alone, quite independently of experience, would lead to the same conclusion. If we consider the enormous size to which the uterus has been expanded before delivery, the extreme extension and consequent tenuity of its muscular fibres, and the violent contractions to which they are subjected, both during and after the expulsion of the child, it is evident that these efforts must greatly exhaust its nervous force, and that it is impossible they can continue so energetic as all at once to obliterate its cavity if the whole of its contents be too rapidly removed. The interior of the uterus after delivery has been compared to the condition of a stump after amputation. Some discussion has arisen as to whether the analogy is perfect or not, for there are considerable resemblances, and some important differences. In one point, however, it is correct, viz., the danger of hemorrhage in both, from open bleeding vessels, which require to be stanched, in the one case by tying or other means; in the other, by a process somewhat of a like kind,—the power which the contracting muscular fibres of the uterus exert upon them. This

power will differ in different persons, but it may be said that, in so far as it is defective, whether by exhaustion from long-continued violent efforts, or natural want of strength, in so far exactly is the danger of after hemorrhage increased. In these circumstances, considering the thing merely theoretically, it would evidently be a great advantage if the uterine muscular fibres, already much exhausted, had something to rest upon half-way, to give time for the recovery of that power which is still necessary, not now, however, for the expulsion of the child, but for the safety of the woman. Of all the contrivances that could possibly be conceived for this purpose, the existence of the placenta, filling the uterus, as it were, like a plug, and applying itself closely to its inner surface, is one of the most admirable. Of the many important functions which it is destined to perform, this is certainly not one of the least; and if nature in her wise designs be not crossed or interfered with, but be aided and assisted, such a course will not only lead with ease to the result aimed at, but will save the practitioner a world of anxiety and much valuable time. These observations might be illustrated by cases in which the partial neglect of the precautions I speak of led to alarming hemorrhage, but I trust it is unnecessary to devote space to them. It would be, of course, to no purpose inserting cases of an opposite kind, illustrating the safety of the practice inculcated, as their evidence would be entirely of a negative character.

(*To be continued.*)

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Art. III.—*Observations on the Intermittent and Remittent Fevers of the West Indies.* By Hugh Croskery, L. R. C. S. I., Honorary Member of the Surgical Society of Ireland; Licentiate of the College of Physicians and Surgeons of Jamaica, &c. &c ; late Assistant Surgeon, R. N.

Mr. R. T., planter, aged 37, of slender make, and rheumatic diathesis, was attacked with intermittent fever on Saturday, the 13th of August, 1859, and, until Monday the 15th, when I saw him, the fever had continued with very little intermission. He stated that he had suffered much on the 12th from headach, nausea, vomiting, and a sense of extreme tension at the præcordium. To relieve the latter, a mustard cataplasm had been applied, and with much benefit. On the morning of the 15th I found him with a moist skin, and a soft but rapid pulse. He did not seem much inclined to talk. The nurse,

however, told me that he had been passing blood, both by stool and in his urine. On examining the contents of the chamber utensil, I found the urine to be of a dark porter colour, with a deposit of grumous sediment at the bottom of the vessel. The evacuations were liquid, black, and very offensive, and evidently contained a large amount of bile. On admitting a stream of light into the room, I now discovered what no one had suspected, namely, that Mr. T. was jaundiced. The conjunctiva were deeply tinged with bile, and the upper part of the body was of a deep lemon colour. The tongue was loaded with a thick yellow fur. The bowels had been moved during the night, and the evacuations were of an acrid nature, showing that the biliary secretion had been more than usually profuse. The attack had evidently been very sudden, and no one had noticed the deep yellow tinge of the skin before I had attracted attention to it. What, then, was the primary cause?

Mr. T. had, for a long time previously, and at different periods, been confined to bed by a rheumatic affection, and had often shown signs of hepatic congestion, passing bloody stools for days together. Just now, however, pressure over the hypochondria and epigastrium does not elicit any evidence of pain. The liver then is not altogether at fault. Mr. T. is often a sufferer from rheumatism, and, as the pathological conditions, which predispose to the latter, are closely allied to the morbid states that induce jaundice and tropical fevers, it is not difficult to come to the conclusion that the jaundice is, in this case, a concomitant of the fever, and that there has been a greater secretion of bile than it was possible for nature to carry off by the usual channel. I ordered him ten grains of calomel and five of James' powder at once, and a draught of five grains of quinia dissolved in a little acidulated water, immediately after; a second powder of five grains of calomel and three grains of James' powder at mid-day, followed by a second draught of quinia; in addition, a Seidlitz draught every third hour, and an opiate of twenty minims of laudanum at bedtime. He vomited a good deal after taking the first powder, and brought up large quantities of bile. In the course of the day the stools became less offensive, and large quantities of dark bilious matter were evacuated. The conjunctiva and skin became clearer, and the patient more lively. About 6 o'clock P. M. I was sent for in great haste, and when I reached the house, I found Mr. T. in a very weak state, and in great pain. The urine was now of a deep port-wine colour, and, on the addition of nitric acid, deposited fibrine in large quantities. A strip of linen dipped in it, and then subjected to the action of muriatic acid,

did not assume the peculiar yellow and then greenish colour, which under this test indicates the presence of bile. He complained of burning pain over both kidneys, but pressure with the hand did not increase his sufferings. Here, again, was another complication—hematuria—doubtless the result of irritation of the kidneys, dependent on the presence of bile in the blood. The tongue was less coated than in the morning; the eye clearer; the pulse 120, soft; and the skin moist. I had him put at once into a warm bath, which had the effect of relieving the kidneys, and of bathing him in a profuse perspiration. He now felt easy, but weak; to have his opiate and a powder, consisting of calomel, three grains, and James' powder, two grains, every third hour during the night. As he complained of excessive thirst, I ordered him draughts of cream of tartar and water, sweetened, as often as he might call for them.

16th. Mr. T. had no sleep last night, and feels very low this morning. The urine is still of a deep port-wine colour, and very scanty; has vomited a good deal of green bilious matter; stools dark and bilious; tongue moist, but deeply coated with a yellow fur; Pulse soft, 104; no abdominal pain on pressure. I ordered a sinapism to be applied over both kidneys; the calomel, James' powder, and Seidlitz draughts to be continued as before, and a tablespoonful of each of the following mixtures every two hours, the one to be alternated with the other: No. 1. Water of the acetate of ammonia, two ounces; spirit of nitrous ether, half an ounce; tincture of hyoscyamus, two drachms; and camphor mixture, sufficient for eight ounces. No. 2. Sulphate of quinia, forty grains; water, ten ounces, and lemon-juice sufficient for solution. Sparkling hock to be given at intervals during the day. I saw him in the evening, and he seemed easier and in better spirits; had some sleep, but had passed no urine; still vomits green bilious matter; but, on the whole, feels stronger. A warm jar to be kept to the feet.

17th. Feels easier this morning; pulse 100, soft; conjunctivæ still much injected; has passed no urine since yesterday, and the bowels have not been moved. A mustard cataplasm to be applied over each kidney, and an enema of warm water, oil, and turpentine, to be given at once; to have also a draught of castor-oil. 4 o'clock P. M. Bowels have been moved, but there has been *no secretion of urine.* To have a vapour-bath at bedtime, and a powder of calomel six grains, and James' powder four grains. The pulse is fuller and stronger, and the skin is moist.

18th. Urine, which, up to this morning, had been entirely suppressed for forty-eight hours, was passed in very small

quantity, and of a more natural colour; the gums are slightly touched; the skin is cool, moist, and clearer; and the conjunctivæ much less injected; bile continues to be passed by stool. *The vomiting has ceased.* He feels inclined to sleep, and appears rather drowsy. In order to stimulate the kidneys, and at the same time to keep the skin moist, I ordered him a mixture of acetated water of ammonia, two ounces; sweet spirits of nitre, half an ounce; and water to make up four ounces; a tablespoonful to be taken every hour: with a draught of bitartrate of potash dissolved in water. This had the desired effect; urine was passed of a natural colour, the bowels were relieved, he became much more lively, and had some quiet and refreshing sleep.

19th. He is much improved this morning in every way.

20th. An officious friend, doubtless with the best intention, gave Mr. T. a strong infusion of senna yesterday. The consequence is that the secretion of urine has been again arrested, and that Mr. T. has been suffering severe griping pains all night. He looks and feels very bad, and is still suffering severe abdominal pain. I gave him a tumbler of warm brandy-toddy, with a drachm of spirits of nitric ether, with much benefit. He soon expressed himself easy, and fell into a quiet sleep. To have a liberal allowance of wine, and the mixture as follows: water of the acetate of ammonia, two ounces; spirit of nitric ether, half an ounce; tincture of hyoscyamus, three drachms; compound tincture of cinnamon and tincture of ginger, of each half an ounce; a tablespoonful to be taken every third hour.

21st. He rested well last night, and the secretions are more natural; but he is still very prostrate.

23rd. He appears to be gaining strength but very slowly. I ordered him eggs beaten up with brandy, in the proportion of one egg to a teaspoonful of the latter, with a little white sugar added, frequently during the day; to have also three grains of quinia, morning and evening. I had him removed yesterday, in a palanquin, from his own house at the works of a sugar estate, himself being the overseer of the property, to the "Great House," which is more cool and quiet than his own, situated as it is on the summit of a hill, and at a distance from the bustle and confusion of the works below.

24th–26th. The fever had evidently assumed a typhoid-remittent type. A slight paroxysm occurs in the forenoon of every day, and continues for about two hours; his stomach has become extremely irritable; he has almost constant hiccough, with frequent eructations of grass-green matter; and, last night, about a teaspoonful of arterial blood was observed

in the matter vomited. The urine, which throughout had been scantily secreted, has now been totally suppressed for the last two days; the hands are cold and clammy; the countenance sunken; and another symptom, *subsultus*, is observed. A naval friend, who happened to be on a visit with me, Assistant Surgeon Francis Greene, of H. M. S. "Valorous," now saw him. We prescribed a mixture of carbonate of ammonia and sulphuric ether with aromatics, in effervescence, every two hours; strong mustard cataplasms, made hotter by the addition of Chili pepper, were again applied over the region of the kidneys; the bowels, which had not been moved, were washed out with a warm enema; and bitartrate of potash was ordered in doses, aperient as well as diuretic; small doses of chloroform to be given in a little sparkling wine at intervals, with the view to check vomiting and allay the hiccough. His thirst, which has been inordinate throughout, has been allayed with iced water. I should mention that I have just now discovered that no quinia has been given to him for the last two days, which is indeed a very unfortunate circumstance. On the evening of the 25th he had slight epistaxis.

27th. Mr. Greene saw him again with me this morning. The alæ nasi are dilated and drawn back; the chin is fallen; the eyes are sunken; green bilious matter is cast up almost without effort, and no urine has been passed. As there appeared to be slight dulness over the pubic region, I passed a catheter into the bladder, and drew off some turbid urine. But he died soon after, without a struggle.

The foregoing case, with its numerous complications, presents many points of interest. Intermittent in the beginning, and with the unusually severe complications of jaundice, bloody urine, and then total suppression of the secretion, it assumed a remittent type of a typhoid character. By the combined influences of quinia and calomel, the ague and jaundice were got rid of, while the local application of mustard relieved the congested kidneys in the first instance, and suitable diuretics restored their interrupted function. I imagined, and with good reason too, that, with the cessation of the paroxysms, and the restoration of the secretions, convalescence would be speedily established; and I have every reason to believe that all would have gone on well but for the unfortunate circumstance of Mr. T. having been drenched with the strong purgative before mentioned. From that day, as will be seen by the report of the case, everything assumed a gloomy aspect: the secretions became interrupted; he was harassed by constant eructation

and hiccough; he had a nervous twitching of the muscles, and almost constant retching; his fever assumed a low remittent type; and he died exhausted. But God forbid that I should, for one moment, think of charging his attendant, who watched him night and day, and showed him every kindness, with his death! His constitution had been impaired by previous disease, and he was a bad subject to bear a wasting fever. In a case such as this, however, where the mucous membrane of the intestinal canal was kept in a state of constant irritation by the presence of acrid bile, and where the kidneys had been but feebly discharging their functions, the exhibition of such a violent purgative as senna would naturally cause a serious amount of mischief. Certain it is that, after its exhibition in this case, the fever assumed a typhoid type, the secretion of urine became arrested, irritability of the stomach with hiccough and constant præcordial pain ensued, and death took place from exhaustion.

Just now, typhoid remittent fever is very prevalent in Kingston, the chief town of the island. It is said to attack the inmates of a house one after the other, and to spread its ravages rapidly through an entire street. Intermittent fever is now more than usually prevalent in this district, and I have lately seen some very severe cases. If the disease is not checked at the onset, the paroxysms become more prolonged and severe, violent head symptoms supervene, and the result is often fatal, more especially in children, in whom, if the paroxysms are allowed to recur, a rapid effusion on the brain takes place, and convulsions end the scene. Apropos of this, I was asked, a few days ago, to see a boy of ten years of age, who was said to have had "hot fever" for twenty-four hours. I found him with a burning skin, a full and frequent pulse, dilated pupils, and violent subsultus. Fortunately, a dose of calomel had been given him some hours before, and a blister had been applied to the nape of the neck. The blister had risen, and the bowels had been relieved involuntarily, as the boy was quite unconscious. I gave him a mixture, which I have been in the habit of prescribing for the last two years with the greatest possible success,—in fact, I never go to see a fever case without it—and, I am now happy to be able, after large experience, to testify to its value in shortening the paroxysms of an intermittent fever, in checking head symptoms, and in preventing fatal results. The mixture is made up as follows:—Solution of the acetate of ammonia, two ounces; spirit of nitrous ether, and compound spirit of juniper, of each half an

ounce; potassio-tartrate of antimony, four grains; tincture of hyoscyamus, two drachms; tincture of opium, a drachm; and camphor mixture sufficient to make a twelve-ounce mixture. Of this I give to an adult a tablespoonful every half-hour during the hot stage, or until what is commonly called "the fever" has subsided; and in no case have I failed to bring it to a crisis after the administration of a very few doses of the mixture. To the little boy I gave a dessert-spoonful every ten minutes, as the case was urgent, and in half an hour I was happy to perceive that a profuse perspiration was bursting out of every pore; small doses of quinia, repeated at short intervals, completed the cure. This boy's parents were expecting his death, which, without prompt interference, would have doubtless taken place in an equally short space of time. As soon as "the fever" has gone off, and the sweating stage has been ushered in, I administer quinia in two-grain doses every hour; and, by getting the system quietly under its influence, I generally succeed in preventing the occurrence of another paroxysm, without producing any of the unpleasant symptoms which large doses are certain to bring on. If a mercurial purgative has not been already taken, I always give ten or twelve grains of calomel to clear out the primæ viæ; and, after the exhibition of this, the quinia is not only much less likely to affect the head, but also much more certain to prevent a relapse. I could instance numerous cases in which, by this treatment, raging fevers, with violent head symptoms, have been cut short, and a recurrence of the paroxysms prevented. In a country like Jamaica, where there are wood and water in abundance, and a burning sun withal, paroxysmal fevers are of very frequent occurrence, and a medical man has daily opportunities of observing and recording. I think, then, it is his bounden duty if, after patient and long trial, he has found a particular line of treatment to succeed, not only in cutting short the paroxysms of a fever, but also in preventing its recurrence, to bring the results of his experience to the notice of the profession; and, in writing this paper, my object is to enable others, placed in a similar position, to do good as I have done. I speak with all sincerity, and I verily believe that, if this treatment were employed generally, we should never hear of intermittent fevers of months' and weeks' duration. Medical men, generally, fall into the grievous error of administering quinia in very large doses; and some even give it during the paroxysms. I have known a case where a colonial physician, in very large practice, prescribed a large dose of quinia for a little girl, and where, on his next visit, in place of finding the case better, as he had anticipated, his patient, who was

thought to be asleep, was discovered to be in a state of complete coma. I think it is also equally bad practice to give quinia during the hot stage. The pernicious doctrine advocated by M'Intosh and Twining, and so loudly condemned by Stokes, of bleeding during the cold stage of ague, has been long exploded; and it is to be hoped that the doctrine of giving quinia, either in large doses or during the paroxysms, will share a similar fate. I have treated patients who begged and implored me not to give them quinia, and who explained to me its effects when given to them in a former fever, and these people have been pleased and surprised to find that their fevers have been cured, and that they have taken quinia without its producing in them any unpleasant symptoms. When this salt is given in a large dose, it produces giddiness, ringing in the ears, and often coma and convulsions. At any rate, it produces a train of symptoms which will prevent the administration of a second dose for a considerable length of time. And what is gained by this? Very little, and sometimes nothing. But when it is given in small doses often repeated, at two grains every hour, it not only enters the system quietly without producing any visible ill effect, but it also cures the fever. In a case, however, of quotidian intermittent, where, during the interval, the patient is left free from fever perhaps only for a few hours, the medical attendant must give a large dose, in order that some impression may be made before the accession of another paroxysm. In such cases, I generally combine calomel with the quinia, say ten grains of each, with much benefit. In any case, however, a dose of calomel should be given, and should be followed up, three hours afterwards, with a dose of sulphate of magnesia, or some such cooling purgative.

In a paper published by me in the "Dublin Hospital Gazette" of August the 1st, 1858, I advocated the same line of treatment, and spoke of it as follows: "I find that this treatment almost always prevents the occurrence of a relapse, and, since I have commenced practice in Jamaica, I have found that, in my hands, it has been most successful in the treatment of intermittent fevers, both in old and young. The acetated water of ammonia and sweet spirits of nitre encourage perspiration, without enfeebling the strength or lowering the nervous system, while the tartar-emetic and henbane relieve the head and prevent delirium." Since then, I have treated numerous cases, and with results the most happy. The addition of a small quantity of laudanum to the mixture is a great improvement. It assists the tartar-emetic in relieving head symptoms, keeping down the delirium, and tranquillizing the respiration.

With the aid of the other ingredients of the mixture, the crisis is soon brought about, the delirium and subsultus subside, the pupils contract, the functions of the skin and kidneys are restored, and the patient is bathed in a profuse perspiration. Having often, while a student in Dublin, seen the wondrous effects of tartar-emetic and opium in controlling the delirium of typhus fever, I was led to employ these agents, in conjunction with the other ingredients of the mixture, in the hot stage of intermittent fever, and I have always found their administration to be attended with success. Some twelve months ago, when I myself was lying on a fever bed, with a burning skin, a full and frequent pulse, tensive headach, oppressive præcordial pain, and insatiable thirst, and at a time when there was no brother practitioner near, and I felt that I would soon lose my reason if relief were not quickly afforded me, this mixture, I am firmly convinced, saved my life. I recollect very well that I drank it in double doses, two or three times within the hour, and, although I swallowed a large proportion of tartar-emetic, I was not in the least degree nauseated. I *felt* that it relieved my tendency to cerebral congestion, and therefore I drank it with avidity. Having been robust and of full habit of body prior to the attack, I suffered much. The fever was very violent; my brain was, as it were, on fire, and I felt my reason leaving me. But, thank God, I was enabled to retain it sufficiently to know that to swallow quantities of the mixture was to save my life. I recollect full well that I took six grains of tartar-emetic within as many hours, and that at last —and it seemed an age to me—"the fever" left me in a bath, as it were, of perspiration.

And now for the intermission. The object here is to get, if possible, a sufficient quantity of quinia into the system, so as to insure an exemption from the attack when the period of the paroxysms returns. This object, then, is best effected by prescribing it to adults in two-grain doses every hour, and by continuing its administration until some cerebral sensation, such as singing in the ears, indicates its effect on the system. This effect should then be kept up until the period of the paroxysm has elapsed, by continuing the dose at intervals of every three or four hours; and, even after this, the medicine should be continued for some days, or as long as any uneasy sensation is felt at the time when the paroxysm should return. In cases of young children, and where there was a tendency to irritability of the stomach, I have given the quinia by enema. I never give it during the paroxysm, as to do so is, in my opinion, cruel treatment, to say the least of it. During the hot stage,

there is, generally speaking, a tendency to cerebral congestion; and knowing, as we do, the sometimes sad effects of this salt, when given in large doses during the intermission, and at a time when the head is cool, this tendency to head symptoms will surely be developed by the smallest dose given during the paroxysm. It is then, in my humble opinion, as bad practice to give quinia in any quantity during the paroxysm, as it is to give it in large doses during the intermission. The effects on the system, when instilled quietly into it in small doses, are by no means unpleasant. I have kept up its influence, in my own person, for three or four days at a time, and the sensation, so far from being disagreeable, is rather a pleasing one.

I must now apologize, not only for the great length to which I have already brought these remarks, but for the infliction on my readers of the following extracts from the journal which I kept while an assistant-surgeon in Her Majesty's Royal Navy. Although not exactly apropos here, they may still be thought worthy of a moment's perusal, as no official account has ever been published of the Bermuda yellow fever of 1856:—

"Bermuda, August 28, 1856.—Yellow fever having broken out in Bermuda, I joined H. M. S. 'Nile' as supernumerary acting assistant-surgeon, having been sent down from Halifax by Rear-Admiral Arthur Fanshawe, C.B., Commander-in-chief. Being a supernumerary officer, I had no duty to perform on board, and, consequently, I had ample time to visit the hospitals on shore. Unlike its previous visitations, yellow fever now attacked principally the native and not the European settlers, although a great many of the latter fell victims to it. Of the Europeans, the ratio of mortality at the Convict Establishment far exceeded that at the Royal Naval Hospital. At the former, the ratio was twenty per cent., and at the latter not more than three. This certainly appears strange. A different mode of treatment was adopted by the medical officers of each institution; and, whether it was willed that divine vengeance should alight on the doomed inmates of the Convict Hospital, as a punishment for the crimes that they had committed, or that the treatment adopted at the Naval Hospital was better suited to the disease, it is evident that the amount of deaths was, comparatively speaking, much greater at one place than at the other. At the Convict Establishment the treatment was as follows: when a patient was attacked, a warm bath and a full emetic were at once administered; and, when the stomach had become quiet, about ten grains of calomel were given to act as a purgative. This was followed up by the administration of two grains of calomel, and a fourth of a grain of opium,

every second hour, until salivation had been slightly established, and stimulants were thrown in as soon as the strength began to fail.

"In the Naval Hospital, the treatment in the first instance was the same, with this trifling exception, that a vapour in place of a warm bath was given. After the emetic and purgative had fully acted, Mr. Rees, D. I. H., the senior medical officer, instead of touching the gums with mercury, contented himself with giving an ordinary diaphoretic and diuretic mixture; and, under this treatment, all cases that were treated in the early stages recovered. Another very important item in Mr. Rees' treatment was the chlorate of potash, which he gave in ten-grain doses, five or six times during the day. On what, then, does the large per-centage of deaths among the convicts depend? It is not my province to solve the problem. I must say this, however, that the results speak for themselves. I should mention that both hospitals are equally well off as far as ventilation is concerned.

"H. M. S. 'Nile' was four months at Bermuda; and, although she lay at anchor all this time, about 500 yards from the dockyard and barracks, where the epidemic was committing its ravages, strange to say, not a single case occurred on board. H. M. S. 'Basilisk' arrived from Halifax on the 12th of November, and, five days afterwards, was sent out to Murray's Anchorage with yellow fever on board. This precaution did not succeed in removing it, as fresh cases were every day added to the list, and, about a week afterwards, she came again within the 'Camber,' where the men were hulked, and the ship thoroughly cleansed. Before the latter expedient was adopted, I have Mr. Rees' authority for stating that no less than seventeen cases had occurred on board."

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Art. IV.—*Cases of Delirium Tremens.* By R. W. O'Donovan, Licentiate of the King and Queen's College of Physicians, Ireland, M. R. C. S. L., Belturbet.

When I state that the little town in which I reside, numbering a population of about 2000, has no less than thirty-three licensed spirit-shops, it is almost needless to add, drunkenness is prevalent, and that cases of delirium tremens are too frequently met with.

This form of disease, to a well-regulated mind, is, I think, the most melancholy "of the ills which man is heir to," whe-

ther considered in its personal or social aspect, or as being occasionally without the pale of the physician's skill.

There is something truly sad and revolting in seeing man, with all his fine attributes, prostituting the image of his Maker, and debasing body and soul in the swinish vice of drunkenness. But as my purpose is not to moralize, but to give the result of my experience, dwelling on those cases which afford points of interest, and slightly noticing others which present little of importance but contrast, I will, without further preface, report the cases which came under my observation.

Case I.—In the autumn of 1841, while yet a young man, and a very young surgeon, I was hastily summoned to attend a young lady, Miss F., I may say my first sick-call to a respectable family. I knew nothing of their private history, being a perfect stranger in the locality. I found Miss F. in bed, surrounded by her mother and some female friends, in a dreadful state of anxiety. She immediately turned her wild countenance towards me, and continued a tirade of incoherent abuse of some imaginary persons. My first impression led me to think my patient was mad, probably labouring under an attack of hysterical mania, but finding from inquiry an absence of any hysterical symptom accompanying the advent of the attack, I was left more in doubt. Her tongue was moist and coated; pulse 110; tremor of hands and tongue; epigastric region tender on pressure; abdomen full. I confess I was "at fault," and fear my countenance was not protected by that diplomatic mask occasionally necessary in certain cases of doubt and anxiety; however, I was quietly called aside by a lady, the widow of an eminent medical man, herself, unfortunately, a notorious drunkard, who, in a very kind manner, said, "Doctor, you do not appear to be aware Miss F. drinks excessively. I think it right to tell you she does!" Well, I was shocked, and bowed my thanks, as the safest way of getting out of the difficulty. She was ordered a bolus of calomel and jalap, to be followed by a turpentine enema; and, when her bowels were freed, half a grain of morphia in an ounce of camphor mixture; her forehead and face to be sponged with tepid vinegar and water.

On the following morning she was much better. She had several large offensive motions; got some tranquil sleep; was rational, unless when excited. She was allowed any light, nourishing food she fancied; her purgative medicine to be repeated, and the anodyne draught to be given as the evening approached. In a few days she was quite convalescent.

Case II.—October 1st, 1842, I was called to see Mr. W., a young gentleman of good constitution, holding a government situation. He resided at one of the hotels, where, meeting a good deal of company, he often indulged too freely.

On arriving, I was told Mr. W. was wandering in his mind all day; that he had locked himself up in his bed-room, and fears were entertained that he would injure himself or some other person, as he had pistols with him, and was constantly exclaiming he would not allow any ruffian to take him; and I was coolly requested to go to his room. Fortunately, we were intimate. When I approached his door, I called aloud to inquire whether Mr. W. was at home, as I wanted to see him particularly. He recognised my voice, and, cautiously opening the door, earnestly inquired whether I had any person with me. On giving him an assurance in the negative, he desired me to walk in, that he wanted a friend, and was glad I came, but to take care and lock the door, which of course I took good care not to do, but put the key in my pocket. He presented a truly lamentable appearance: the impersonation of excitement, fear, and cowardice; face flushed, bathed with perspiration; eyes blood-shot; hands and limbs trembling. Going to the far end of the room, he commenced a story of an imagined attack, and his determination to resist it. To convince him it was a delusion, was truly labour in vain; so I suggested the prudence of his leaving the place with me, and that, while putting up his things, I would keep guard with the pistols; to this he at once assented. After a little time spent in confused hurry, I said, "Suppose we have a little refreshment before we go?" This was the greatest difficulty. The fear of opening the door, and admitting the supposed assassin, took possession of his mind with redoubled terror. However, after great patience and tact, he consented, and I succeeded in getting in some ale, with a grain of tartar-emetic dissolved, which he drank off greedily. Almost immediately he became exceedingly sick, throwing off quantities of yellow and green bilious matter; but he also became much more tranquil and manageable, and lay on the bed, continuing to watch and stare about the room, sometimes covering his head in the bed-clothes, and all the time speaking in a subdued, frightened tone. Sitting by his bedside, and anticipating his fancies, I succeeded in giving him 5 grains of calomel, and, in two hours after, half a grain of tartar-emetic, with 10 minims of Battley's sedative solution of opium, in a glass of cold water. In a little time he became again sick, and the cold perspiration poured over him. I gave him half a glass of weak hot punch, which he readily took; gradually he became quieter, though

he did not sleep. In two hours his draught was again given in punch; it did not sicken; he talks a great deal less; his skin is warmer, and his manner not so frightened. The room was darkened, and after a little muttering talk, he fell into a quiet but not sound sleep. I remained with him, and in about an hour he awoke, something quieter. The calomel now operated freely; in another hour I repeated the draught of tartar-emetic and opium; he slept after it, and I left him to the care of a friend.

October 2nd. Mr. W. is much better; he slept for three hours; skin warmer; pulse 100; tongue soft, and countenance more natural, yet he feels tired; abdomen full and hard; made water freely, high coloured; to get 5 grains of calomel and colocynth, with chicken broth, a little punch, and an anodyne at night. In a few days he was quite well. Shortly after his illness he was removed from this locality, and I lost sight of him.

Case III.—September 2nd, 1844, at 10 P.M., I visited Mr. J. M'H., a publican; unmarried; middle-aged; saturnine temperament; had met some reverses in worldly matters, and for some time previous to my visit had been drinking very hard. I found him in bed, with a most lachrymose expression of countenance, muttering penitential expressions of remorse, but occasionally bursting into loud abuse of the members of his family, domestics, and himself, heaving heavy sighs; with a quick and small pulse; tongue tremulous, moist, and very much coated; clammy perspiration over entire body; tremors of hands; face and head hot, but not the "calor mordax" of continued fever; had no sleep on the previous night or day; bowels confined; thirst very great; stomach irritable; had repeated discharges of green bile during the last twenty-four hours. He never had an attack of this nature before. Ordered a bolus of calomel and jalap; soda-water as a drink: so soon as the bowels were moved, the following draught to be taken:—tincture of opium, 40 minims; camphor mixture, an ounce.

3rd morning. The medicine did not act freely; he had a little disturbed sleep; the tremors are rather increased; tongue dry in the centre; he is rather more irritable, constantly turning in the bed, but does not desire to leave it. There is considerable epigastric tenderness; no pain of head, nor does he complain of light or noise. Bolus to be repeated, and followed by turpentine enema; sinapism to epigastrium.

Mid-day. Copious foul discharges from the bowels; epigastric tenderness less; no change in other symptoms. Anodyne to be repeated in half a glass of punch.

Evening. He is more tranquil, but has not slept; got in my

absence some more punch, which has appeared to have agreed with him; to get 12 minims of Battley's sedative solution in half a glass of punch.

4th morning. Had a tranquil night, and some sound sleep; all his symptoms are improved; to get a mercurial aperient, nourishing diet, some punch, and the opiate draught to be repeated at night.

7th morning. Up, and quite well.

Case IV.—June 8th, 1849, I again attended this man.

Case V.—Again, in May, 1850.

Case VI.—A fourth attack, in April, 1854.

Case VII.—A fifth, in December, 1854.

The symptoms in each illness varied so little, except that the nervous symptoms—delirium and tremors—were more violent, and the treatment required a freer use of stimulants and opiates, and he progressed more slowly towards convalescence, there would be no object in giving a detailed account of each attack. This unfortunate man possessed a good appetite, and enjoyed good health in the intervals of his drinking fits. He was not a "tippler," and frequently for a long period abstained entirely from the use of spirituous liquors. He terminated his career in January last. After my last attendance on him, in December, 1854, I ceased to have any professional knowledge of his habits, but am aware he became a confirmed drunkard, rapidly put up fat, and got a stupid and squalid look. He had several attacks previous to his last fatal one, and in one of them his medical attendant informed me, he suffered from, first, suppression, and afterwards, retention of urine.

Case VIII.—On April 26th, 1850, at 5 o'clock A. M., I was called to see Mr. J. D., a fine young man, but unfortunately addicted to fits of intemperance. I found him in a very ludicrous position indeed—standing in front of his father's house, he was supporting it from falling, calling aloud to the persons about him not to approach it. For this laudable purpose he had a board, about two feet in length, leaning against the wall, with his foot firmly placed on it! Fear and anxiety were strongly depicted in his countenance, his whole body trembling with excitement. After a considerable time, with great persuasion and tact, he was induced to go home, but nothing could induce him to go to bed, or even to sit on a chair; he persisted in walking about the room, everything in turn appearing to his diseased mind in the act of falling. I gave him half a grain of tartar-emetic in a drink of cold water, the only way he could be got to take anything. This was repeated during the day at uncertain intervals, in fact, just as he chose to drink; towards

mid-day his stomach got sick, and he threw off a considerable quantity of bilious matter. To continue the tartar-emetic; two grains were dissolved in a pint of cold water, and left near him to drink as he thought fit; for, if asked to have a drink, he invariably refused. At nightfall his stomach again sickened; he now took some soda water, at his own request, to "settle his stomach." I managed to give him 40 grains of tincture of opium with it; he was coaxed to bed, and became, after some time, a little tranquil. He got during the day four grains of tartar-emetic.

Two o'clock A. M. Now, for the first time, could I get him to put out his tongue, which was white and moist; pulse 100, soft; warm perspiration over face and body; hands clammy and cold; though not desiring to get out of bed, he continues much afraid that everything in the room will fall, and hurt himself or some other persons, and he frequently shouts suddenly to some of his attendants to beware of the falling of some imaginary beam, or the bed-post, &c., &c., putting out his trembling hand to snatch them from the danger. To have his opiate draught repeated every three hours, unless he sleeps.

April 4, 10 A. M. Mr. J. D. had a couple of hours' rest, and is much better; does not talk so incessantly, nor are his imaginations so vivid; he starts occasionally, but recollects himself almost immediately: ordered blue-pill, with senna draught, and the opiate to be repeated every six hours: allowed chicken-broth and bread.

5th. Is considerably better: to repeat his aperient medicine, and get his opiate at night.

6th. Convalescent.

The previous history of this young man has some points of interest, which, though not legitimately within the scope of this paper, may, I think, be introduced as not unworthy of observation. For some years he resided at one of the West India islands, and while employed on the roof of a house, superintending repairs, he fell, alighting on a beam of timber. The accident was followed by violent inflammation of the scrotum; suppuration and sloughing supervened, with eventual loss of one testicle; he also suffered from difficulty in passing water, which gradually increased. Shortly after he returned home (for the benefit of his health), he contracted gonorrhœa, and consulted me. He was cured without much trouble, and without having recourse to injections. Some few months after, he again got gonorrhœa, and being ashamed to come to me, he put himself under the care of "a quack," who used strong astringent injections, and cured him in half the time the doctor took. This

treatment was followed by increased difficulty in micturition; and, in December, 1856, he consulted me for permanent stricture. I then became acquainted with the facts detailed. On examination I found a firm stricture about four inches from the orifice of the urethra; its hard membranous nature could be distinctly felt externally, very painful on the slightest pressure: the point of the finest bougie could with difficulty be introduced, and, when introduced, was always followed by an aggravation of his symptoms. In consultation with the late Dr. Roe, of the County Cavan Infirmary, who saw him with me several times, he was leeched, fomented; belladonna applied externally and internally; calomel and opium given, but without relieving the constitutional irritability, or subduing the inflammatory action; he now suffered a great deal, from forcing to make his water; from constant irritation, and loss of rest, he became pale and hectic, with loss of appetite and spirits. It was also evident, from the expansion of the urethra behind the stricture, that he was threatened with urinary abscess. Having consulted with some medical friends, it was determined to open the urethra from the perineum, which operation I performed, assisted by Dr. Kendall, of the 17th Lancers, on the 7th February, 1847. He bore the operation well; a silver catheter was passed into the bladder, and the wound healed over it; he never after required the introduction of a catheter.

Case IX.—Feb. 5th, 1852, I was called again, at an early hour, to see Mr. J. D., whom I found coiled up at the far side of his bed, which was placed close to the wall, in a shocking state of fright. When I approached to speak to him, he exclaimed, "For God's sake, Doctor, don't lean on it; if you do, it will tumble over, and we will both be killed." He was under the impression that he was on an outside car; that one of the wheels had got off; that he was balancing it, and that if he changed his position, or any one leaned on it, the car would certainly topple over; he was bathed in profuse perspiration; his hands tremulously clutching at the bed-clothes; he had been incessantly drinking for the previous week, and was wandering in his mind the entire day, but his companions, however, contrived to get him to bed, where I found him. His treatment consisted of solution of tartar-emetic, calomel purgatives, and opiates, with spirits and water occasionally. On the third day he was convalescent.

So far, my experience of delirium tremens had a happy termination. The subjoined cases, I regret, terminated fatally: each was complicated with convulsions, one being preceded by epileptiform fits; the others terminating in violent muscular

spasms. Two of them might be classed under the denomination of delirium traumaticum, having been preceded by injuries which, though slight in their nature, yet, in the debilitated state of the constitution of the individuals, were productive of the most serious consequences.

The case of Mr. J. M. is already recorded in the Dublin Hospital Gazette for 1857, page 182. He was a regular "tippler," drinking, as a daily habit, half-glasses of brandy and whiskey, and, in fact, living on ardent spirits; his appetite for ordinary food having been entirely lost for many months previous to his fatal illness, he never ate an ounce of food at a meal. He never previously had a direct attack of delirium tremens, but he was easily excited, and his temper became ungovernable; his personal appearance was also changed; from being a light, athletic man, he became gross and flabby.

Case X.—On the 18th February, 1857, I was urgently requested to see him, having, when ascending the stairs, fallen backwards, and received a slight contused wound on the occiput, from which he lost about two ounces or less of blood. With my assistance he walked up stairs, undressed, and went to bed. A little adhesive plaster was put to the wound; during the night he had some bilious vomiting, but did not complain of headach, except some pain in the cut.

At mid-day on the 19th he was attacked with well-defined epileptiform convulsions, which ultimately yielded to tartar-emetic and tincture of opium. He continued to do well till the night of the 21st; and at 7 o'clock A.M. on the 22nd, I was hurriedly sent for. He was exceedingly unmanageable; could with difficulty be kept in bed, being haunted by spectral illusions. The expression of his countenance was remarkably silly; pulse tremulous, 102; ordered half a grain of muriate of morphia immediately, which did not produce any sleep. At 11 A.M. all the symptoms much worse; pulse 120; he was frightened at everything, fancying all manner of horrible things; constantly shouting at the loudest pitch of his voice. I ordered an ounce of the following mixture every hour and a half:—tartar-emetic, four grains; tincture of opium, half a drachm; camphor mixture, and water, of each, 4 ounces. At half-past 3 o'clock P.M., having got three doses, he threw off a considerable quantity of bile; but no sleep has been procured. In consultation with the late Dr. Halpin, of Cavan, a blister was applied to the epigastrium, and twelve minims of Battley's solution of opium given; and as he was exceedingly violent, and determined to put on his clothes, he was allowed to do so, care being taken to have two men to watch him; he ate some bis-

cuit, and drank a little brandy and water during the day. 10 P. M. No sleep; to get 15 minims of the solution of opium; continued walking incessantly through the room, occasionally stopping to peep through a small hole in the wall, watching most anxiously every transaction he supposed was going on in his establishment. At midnight there was no change for the better; he looks much exhausted, but is quite as unmanageable; the opiate to be repeated.

23rd. Had another opiate at 5 o'clock A. M., but without any effect; ate a little bread and milk (which was left in his way) by stealth. 2 o'clock P. M. Has taken two draughts, but without any effect. The blistered surface has since morning become exceedingly foul, and a dark patch is appearing where the sinapisms were applied to the calves of his legs during the epileptic attack. At 9 o'clock P. M. we determined to give him 30 minims of chloroform; at half-past 9 o'clock he got 35 minims; at 10 o'clock, he got 40 minims; Dr. Halpin and myself remaining all the time in the room with him. At 11 o'clock P. M. he appeared drowsy, though continuing to walk about; the vital powers suddenly became alarmingly depressed. He allowed himself to be laid on the bed, and took half a glass of hot punch at a draught. At this moment his pulse became scarcely perceptible. He continued exceedingly restless, but with great tact he was kept in bed, and at 1 o'clock A. M. sleep gradually came on, but only in snatches, and he awoke nearly as unmanageable as ever. Towards morning he became more quiet, and was induced to take about an ounce of brandy in water.

24th. Continues tranquil; sleeping occasionally; and taking brandy in considerable quantities; is rational when spoken to; but his pulse can hardly be felt; the heart's action is normal, but the impulse very feeble. To continue brandy, with chicken jelly. In the evening his pupils became contracted; his breathing very slow when asleep, from 4 to 6 in the minute; he had no moaning or starting. He lived until 2 o'clock A. M. on the 25th without any perceptible change, his breathing becoming accelerated to 12, but easy and equal. He died without convulsion or struggle.

Case XI.—June 12, 1858, 8 o'clock P. M., I was called to see Mr. P. M., a shop-assistant, aged 38, of intemperate habits. While under the influence of drink, he was knocked down in a quarrel, and received a lacerated wound on the right side of the head, which divided the temporal artery. He bled profusely before I saw him. A compress, saturated with collodion, was

applied with a bandage; some wine was given him; he was put to bed; and cold applications applied to his head.

13th. Had a good night's rest; pulse 80, weak, but tranquil. To get some mutton-broth, and wine and water, and remain in bed.

14th. Had a good night's rest; examined the wound; it is quite healed. Cold lotion to be continued, and to remain in bed as a precautionary means; to have a chop and wine.

15th. Was called at 5 o'clock A. M.; found Mr. M. quite delirious; he spent the entire night roaming through the house; frantic and perfectly unmanageable; face flushed and covered with perspiration; eyes wild; pulse 120, weak; great muscular tremors; constant delusions; he sees parties getting over the wall to plunder the house; and can with difficulty be kept from running down stairs or through the window to prevent them. I learned that his bowels were moved, and that he made water freely during the night. With great difficulty he was got to bed. I gave him thirteen minims of black drop, with a quarter of a grain of tartar-emetic in one ounce of wine and water. I sat at his bedside, and, fortunately, had sufficient influence over him to keep him in bed; he even kept his eyes shut (pretending to sleep) to gratify me, but, when so doing, he never ceased muttering and occasionally shouting about the supposed thieves. At 7 o'clock A. M., there being no improvement, though he feigned sleep, his draught was repeated. I now left him in charge of his friends, and returned at 10 o'clock A. M., when I found him dressed, unmanageable, and violent; harassed by the same phantoms, and bathed in profuse sweat; pulse exceedingly small, weak, and quick. To get a grain and a half of opium every fourth hour with beef-tea and wine. 10 o'clock P. M. He is not so frightened, nor is the perspiration so profuse, but he has *not slept;* pulse steadier, about 110; opiate pill repeated. Half-past 4 o'clock P. M. Perspiration decreasing; eyes less wild; pulse 80. I have kept him in bed; he does not appear so wild; is quieter and more rational; met the late Dr. Roe in consultation, and at his request the opium was decreased to one grain every sixth hour; to continue his arrow-root, beef-tea, with spirits and water.

15th. Was called at 7 o'clock A. M. Mr. M. is seriously changed for the worse; he spent the night wandering about, all his horror of robbers having returned; countenance pale and anxious, with cold perspiration over him; pulse exceedingly quick and weak. Having succeeded in getting him to bed, I gave him two drachms of tincture of hyoscyamus, and had admi-

nistered a cold shower-bath, which he enjoyed; he was dried with a hot sheet, put to bed, and got some hot punch. For an hour he remained quiet in bed, during which time I sat by his side, but other professional duties obliging me to leave him, I found, on my return at 12 o'clock, that I had no sooner left than he again got up, and became quite unmanageable. Ordered one drachm of the following mixture every three hours: Muriate of morphia, 2 grains; dilute muriatic acid, 20 minims; sweet spirits of nitre, 4 drachms; water, 7½ ounces. Got his medicine, and took some beef-tea; cannot be kept in bed by his friends; is much worse; and is rapidly losing strength; pulse fluttering, cannot be counted; every fibre is trembling; never ceases talking; continued getting worse till 11 o'clock P.M., when he died, in violent muscular contortions.

Case XII.—February 7, 1859. At 9 o'clock P.M. I visited the subject of the following case:—Mr. C., a respectable young man, kind, humane, and honourable, beloved by every one who knew him; he had but one vice—irreclaimable drunkenness; his unfortunate end was often foretold to him by many of his friends; among them myself; but his promises of amendment "were made but to be broken." For many months previous to his fatal illness he was observed gradually getting up *fat*, while his general health visibly declined; he suffered from tremors of his hands, profuse perspiration from the slightest exercise, frequent diarrhœa, with bloody stools, night-sweats, loss of temper and appetite; and he also acquired a habit in conversation of repeating over and over the same matter, subject, or incident. For four days previously to my seeing him he had been drinking incessantly, and eating very little, if any, food. On entering his room, I found him sitting up in bed, his servant holding him round the waist; a heavy stick in his hand, defending himself from some imaginary ruffians, whom he supposed were at the foot of the bed; suffused with perspiration; his eyes standing from their sockets; his hands dripping with cold perspiration; pulse 120, small and weak: his bowels were freely moved during the day from a dose of castor-oil. As soon as possible, a cold shower-bath was given to him; he was wrapped in warm sheets, and put to bed, and twelve minims of Battley's solution of opium, in a little warm punch, given to him. For an hour he was more tranquil, though he continued his fixed and frightened stare towards the foot of the bed; his draught was ordered to be repeated at 1 o'clock A.M. if he did not sleep. I left him at 12 o'clock. Before 2 o'clock A.M. I was again sent for; he has become quite outrageous; two of his friends who have sat up with him have lost all control

over him, and have been obliged to use their united strength to keep him quiet; he immediately lay down for me; six leeches were applied to his temples, his hair cut off, and cold lotions applied to his head; it is almost impossible to count his pulse, so weak and rapid is it. He got five grains of calomel, to be followed by an enema. He took a little bread and tea, but the entire time his whole frame appeared as if under the action of a series of galvanic shocks, and he never for a moment ceased speaking; spectral illusions constant.

I had the able advice of Dr. Mease (Cavan Infirmary) at 9 o'clock A. M. Before his arrival, he made water copiously, of a dark red colour, similar to the urine in bad petechial fever. It was agreed to give him chloroform, with acetated tincture of opium, a turpentine enema, beef-tea, and stimulants according to the necessity of the case. He was carefully attended during the day, and, though rational occasionally, he continued to sink. As evening approached, his mental illusions became incessant; a stream of perspiration flowed from him; his tongue never for a moment ceased; he appeared as if speaking to every person he ever knew, addressing them sometimes as present; again telling confused anecdotes, breaking off, and mixing one passage of his life with another; constantly searching for things in the bed-clothes, folding them up as parcels, again drawing them up, fancying he was putting on his stockings and trowsers. He died at 5 o'clock A. M., after suffering four attacks of the most frightful convulsions I ever witnessed: lying on his back, his legs and arms were stretched upwards, and his entire frame worked with a frenzy of convulsive action perfectly appalling to witness. For about half an hour immediately previous to his decease, he lay quiet, as though nature was quite exhausted.

One of the greatest difficulties accompanying the study and treatment of this painful disease arises with the questions—whether it owes its existence to functional or structural lesion? which function takes a predominant place, or which structure is primarily affected? whether it is inflammatory or the reverse?

I believe it is essentially a functional disease, but whether primarily of the stomach and liver, invariably found affected, or of the cerebro-spinal system, as certainly found engaged, I hesitate to give a positive opinion, but am inclined to view it as having its predisposing cause in a vitiated state of the biliary system. In every one of the cases I have seen, vomiting, whether spontaneous or the result of medicine, was always attended with large quantities of blue, green, or yellow bile; and,

where the case was not complicated by a broken-down constitution, or some accidental depressing influence, as severe loss of blood, such as preceded the attack with Mr. P. M. (Case XI.), the nervous system was readily brought under the influence of anodyne sedative medicines. Mr. J. M.'s case (No. X.), ushered in by epileptiform convulsions, treated as an inflammatory affection at first, with mercury and blisters, very quickly yielded to the combined influence of tartar-emetic and tincture of opium, so soon as the primary effect of the tartarized antimony, vomiting, accompanied by large doses of vitiated bile, took place. No doubt, inflammation of the brain or its membranes may sometimes accompany true delirium tremens as well as any other disease, but, that inflammatory action is essential to the existence of the disease, or necessary to its full development, I do not believe; first attacks of this disease while the constitution is yet unimpaired, readily yield to emetics, purgatives, and anodynes, and seldom last beyond two or three days. In those cases one would naturally look for evidence of the existence of meningitis, cerebritis, or some other form of inflammatory action, but I never found a patient in delirium tremens complain of "intolerance of light," though the eye was often suffused and blood-shot; neither of "intolerance of sound," or pain in the head, at least of that persistent "corded" pain so characteristic of inflammatory action within the cranium. But, again, there is another interesting fact in connexion with the delirium and other nervous symptoms in delirium tremens: I allude to their remittent character, which, indeed, appears to me to be nearly symptomatic of this disease. I never saw a case in which the nervous symptoms were not vastly *worse at night;* yet this is not the case with inflammatory disease, when the delirium, &c., does not abate until the inflammation begins to yield to treatment, nor does the result of treatment favour the idea of inflammatory action. Depletion, whether accidental or remedial, is badly borne. Mr. P. M. (Case XI.) lost a great deal of blood from the wound in the temporal artery; but the third day after the accident he was attacked by delirium tremens, which proved fatal: and in Mr. C. (Case XII.) the application of six leeches to the temples (though the head was hot, and the eyes suffused and blood-shot) was followed by manifest increase of all the bad symptoms; and, though purgatives and emetics are well borne, and attended with positive benefit, yet, unquestionably, their remedial effect is not to be attributed to their depletory or anti-inflammatory action, but to their efficient removal of vitiated secretions.

From practical observation I am forced to the belief that

delirium tremens is a disease of debility. In the victims to the inordinate use or abuse of alcoholic drinks, I have observed a gradual decrease in the normal functions of every organ in the system, a rapid wearing out of vital energy, loss of appetite, loss of rest, deterioration of the muscular system, and its replacement by fatty matter, loss of tone in the exhalents, vitiated biliary secretion, loss of muscular power in the intestinal canal, evidenced by frequent diarrhœa, loss of memory, want of tone, both in the voluntary and involuntary systems. How alcohol produces all these destructive results, I am not able to offer an opinion, but I have seen it paralyze the vital action of the entire human system, in at least one case. About two years ago I was called at 10 o'clock A. M. to visit a young man, residing in the county, aged 18, married three days; he had been in town with the wedding party, who drank a great deal; some of his friends sickened on the road home, but he did not; he was not apparently drunk; but on arriving home, he complained of great weakness in his limbs and back, and had to be assisted to bed. From what I heard of the case, I took with me an enema apparatus and catheters. I found him in bed, complaining of great pain in his spine and abdomen; no pain elsewhere. I learned he had not made water, though he tried at different times on his way home. I found his bladder empty; I administered an enema, which brought away some hardened fæces, and had him put into a hot bath; ordered a bolus of calomel, croton-oil, and jalap, with a stimulating liniment to be rubbed to the spine. I did not see him again, but I learned from an intelligent man that the debility in his limbs increased, so much so that he could not turn in the bed; he made no water, there was no action of the bowels; he became comatose early in the morning, and died in the course of the day, a victim to whiskey poison. In all probability the immediate cause of death was the presence of urea in the blood.

The aggravation of the symptoms of delirium tremens during the night, and their partial remission during the day, is a matter of great moment, which never should be lost sight of. This day-remission in bad cases, and in these it is more marked, affords the only opportunity of successfully struggling (if possible) with the ravages of the disease. The great danger is lest the practitioner should be satisfied with the partial improvement, and slacken his energy and watchfulness. I fear in Mr. P. M. (Case XI.) this error was committed; a careful perusal of it will show a gradual abatement of all the bad symptoms under the steady use of opium in $1\frac{1}{2}$-grain doses every fourth hour, when his pulse sank to 80, perspirations less, he was

more rational and quieter, though he had not slept, and sleep was all which was wanted to secure his recovery. My friend, Dr. Roe, seeing him at this particular moment, 6 o'clock P. M., could not believe he had passed such a night and day; and, judging from the case as it was before him, considered one grain of opium every sixth hour sufficient. The alteration was made at a critical moment in the progress of the case, as well as in the advance of the day; of course I acquiesced in the change of treatment, which I believe was a fatal error of judgment, and tended to a fatal result; and this leads me to a remark or two on the dread which appears to haunt some practical men in the use of opium. I believe it is a very valuable medicine, which can be used with benefit in much larger doses than usually prescribed. In acute rheumatism, neuralgic disease of the spine, uterus, painters' colic, &c., and particularly in delirium tremens, where we have a great object to be gained, as well as a great difficulty to be overcome, the production of sleep, which is essential to recovery, I believe there is greater risk in timid practice than in boldly administering the drug.

I have tried the cold shower-bath in the manner recommended by Dr. Law in the pages of the Dublin Quarterly Journal, but without deriving the amount of benefit his success led me to anticipate. It cannot, however, be said to have got a fair trial in my hands: it certainly produced temporary alleviation of the bad symptoms, but, after an hour or so, reaction set in, and the patient became worse. That it is a valuable sedative, I have no doubt, and, adopted in cases which yet hold out a hope of recovery, may be attended with good results, but in subjects presenting fatty degeneration with loss of appetite and long-continued tremors, though free from any regular attack, neither the cold shower-bath nor any other treatment will have much chance of success.

In private practice it is almost impossible to manage those cases; nevertheless, judicious control will be found one of the surest means of recovery; friends and relations seldom have the energy and tact requisite, nor can they be persuaded of the necessity which exists for quiet determined management; the patient, too, will be more disposed to yield to the firmly expressed will of strangers. I have myself found it comparatively easy to bring the most headstrong to subjection, and during my presence keep them quiet; but at my next visit I have been invariably met with the news, "Oh! Doctor, the moment you left we lost all control over him." This is the more to be regretted in the country, where it is impossible to obtain skilled nurses.

Art. V. — *Practical Deductions from an Experimental Inquiry into the Influence of Foods.* By Edward Smith, M. D., LL. B., L. R. C. P., Assistant Physician to the Hospital for Consumption, &c., Brompton.

Whilst engaged in a lengthened series of inquiries into the influence of foods over the respiration, which have been published in the Philosophical Transactions for 1859, various facts have been observed which are of interest to medical science; and, although the inquiry is a physiological one, it has afforded data calculated to direct us to practical truths.

The experiments which have hitherto been made as to the influence of individual articles of food have been almost exclusively made upon animals, with a view to determine their influence upon growth and fattening, as in those admirably conducted by Boussingault, and Messrs. Lawes and Gilbert of Harpenden; or to determine how long an animal would live upon one kind of food, as sugar for example, exclusively. A few have been made upon man,—as those of Prout on alcohols; Lehmann on tea and albumen; Vierordt on white wine; and Böcker on tea, coffee, sugar, and alcohol: but in respect of most of these it may be remarked, that the inquiries were very limited in number; were not serial, as those of Vierordt and Prout; were not made at the only period of the day when the uniform state of the functions permitted the investigator to trace the changes due to the food alone,—as those of Böcker, Vierordt, and Prout; were made with excessive quantities of the food, as when Lehmann ate thirty-six eggs, and Böcker ate large quantities of sugar repeatedly; and were not made whilst the ordinary quantity of ordinary nutriment was taken daily.

Hence much that is known is not applicable to the determination of the daily wants of the body in man; and, of the rest, much is exposed to error from the defects in the method adopted. The greater part of the knowledge which we possess, and upon which we at present rely, has been deduced from the chemical constitution of the various kinds of food, and in this respect we are chiefly indebted to Liebig, the most sagacious chemist of the day; but even chemists are not slow to admit that much of this kind of knowledge is hypothesis; which, if true, yet needs the help of additional proof, and, when admitted to the full, fails to explain the transformations which the food undergoes in the body, and the precise mode in which it nourishes the animal frame.

Under these circumstances, I think it necessary to state,

that the observations contained in the following paper are exclusively derived from my own experiments. These experiments (2000 in number), in many cases, confirm previous knowledge, by adducing proof which had not been heretofore derived from the human system, whilst in others they tend to correct error, and in many give explanations of known facts, as, for example, the inferior value of starch to wheat-flour as a nutrient, not alone (as has been hitherto supposed) because the latter only contains material to build up the tissues, but, as I have shown, because nitrogenous compounds, when present, promote the transformation of starch.

Whenever my results differ from those obtained by previous observers, I offer them with all deference, but at the same time with confidence; and refer to the fact that, up to the period of my experiments, there was neither opportunity nor method devised by which such experiments could be made in the accurate and serial manner which is now possible. I venture also to hope that, where my observations refer to and support facts which are known, they will not be deemed out of place, seeing that they are based upon new proofs of our knowledge.

As I do not propose to enter into detail with reference to the experiments, I must refer to the Philosophical Transactions for a description of the method employed and the results obtained. The aim was to determine the effect of various kinds of food (each taken alone and in a moderate quantity, and all under precisely similar circumstances) upon the respiratory function and pulsation, and also upon some other functions of the body. The effect was isolated by taking the substance when fasting, before breakfast, and in a state of perfect rest, and by repeating the experiments every twelve to fifteen minutes during its action. The persons subjected to inquiry were chiefly myself and Mr. Moul; but Professor Frankland, F.R.S., Mr. Hoffman, of Margate, and Mr. Reid, of Canterbury, all in perfect health, kindly lent their aid. The apparatus employed for the collection of the carbonic acid is described and figured in the papers above-mentioned.

Before entering upon the subject, I propose to offer a few introductory observations.

It is known that a large amount of vital action must be maintained in order to preserve life, so that in cases of starvation the sufferer does not continue to live until every vital change is progressively lowered to zero; but, on the contrary, dies whilst there is still a large amount of vital action. This is not explained even in cases of starvation by stating that the

stock of nutriment is exhausted, since it would be more rational to assume that, as the stock diminished, so would the vital actions diminish until both subside to zero, and in the present state of our knowledge we can give no other explanation than to say that it is one of the properties attaching to life. The determination of this *necessary* amount of action is therefore both interesting and practically important, since it shows to us the reason for the rapid exhaustion of the system in the absence of food. Cases have occurred in which life has been maintained for some weeks, both in health and disease, in the total absence of food, except the important one of water; but it is well known that privation of food is a most powerful and urgent agent in the hands of both the physician and the magistrate, and over both the mind and the body. In my experiments I did not feel justified in fasting longer than twenty-seven hours (or rather forty hours, with only a moderate breakfast in that period), for, had I proceeded further, I should probably have set up some action allied to disease. Two facts were noticed of interest to the physician, viz., that in this total absence of food the amount of respiratory action was still seventy-five per cent. of that which would have occurred in the twenty-four hours with food; and, second, that the respiratory function was almost absolutely uniform through all the hours of the daylight day—that is, at the periods when the variations with food are extremely great. In my case, the total quantity of carbonic acid exhaled during the twenty-four hours of fasting (the body being at rest) was nearly twenty-two ounces, containing a quantity of carbon equal to that found in twenty ounces of bread. This loss would soon exhaust whatever excess of nutritive material might have been previously found in the system, and hence the withholding of food is, as has been long known, the most natural and efficacious remedy in some forms of dyspepsia, disorders of the liver, and inflammations, and in numerous cases of threatened disease.

It has been observed in experiments on animals, that, from the moment when food was withheld, there was a gradual and progressive decline in the carbonic acid emitted until it was reduced below the amount observed in my case. In myself there was not such a progression during the period of the experiment; but, after having fasted twenty-two hours, the quantity of carbonic acid evolved was precisely that which had occurred at seven hours on the preceding day, and the total variation in the twenty-seven hours did not exceed half-a grain of carbonic acid per minute above or below the quantity re-

corded five hours after the last meal. Hence, uniformity of vital action is the most remarkable characteristic of a fast of this duration. The general effect upon the system was well marked, since the blood became unduly alkaline, as evinced by an alkaline state of the urine, and an increased degree of alkalinity of the saliva, which were noticed after twenty-five hours[a]. The urine was of specific gravity 1018, and high-coloured. The pulse was very soft, and wavy or jerking, with throbbing in the head, and a most unpleasant headach at intervals, depending, doubtless, upon the feeble action of the heart and lessened quantity or changed character of the blood. There was no marked sense of craving for food or drink, but, succeeding to a feeling of great tameness, was a peculiar kind of irritability, which is the prelude to the delirium known to attend inanition, and which, as Dr. Copland has so well remarked, has been often induced by the exhausting treatment of disease. The alkaline state of the blood whilst fasting is much greater than the degree of it which follows the administration of food, and has a peculiar significance in relation to Baron Liebig's views as to the carrying powers of blood in reference to the carbonic acid, and must be attended by diminished respiratory changes. Having ascertained the fact of the increased alkalinity, I was desirous to learn if the introduction of acid, which is capable of being resolved into carbonic acid, would increase the chemical changes and diminish the alkalinity of the blood; and therefore drank six drachms of good vinegar in water; but only a slight and temporary change ensued. There was, however, a fact which is worthy of note; viz., that the unpleasant sensation in the head ceased almost instantly, and the pulse became a little quicker and sharper; but after the lapse of a few minutesthe improvement subsided[b]. Arrow-root gave no relief,

[a] It has been objected that the state of the excretions is not sufficient evidence as to the degree of alkalinity of the blood; but I cannot think that any one who, with sufficient knowledge, reflects upon the subject, can hesitate to adopt views similar to those expressed by Liebig, both as to the constitution of the blood and the mode by which it carries on its functions through a perpetual interchange between various acids and the bases in the blood, and that he must admit there is such a correspondence between the state of the urine and other excretions with the blood, that the condition of the latter may be reasonably inferred from the former. If, for example, we find an unusual quantity of acid passing off by the urine and the perspiration, not connected with an unusual ingestion of acid, whence could it have been derived but from the blood which contained it in excess; that is to say, in greater amount than could be transformed by the aid of the alkalies then existing in the blood?

[b] Although acetic acid is, as above stated, capable of being resolved into carbonic acid, it has yet the power of modifying the degree of alkalinity of the blood, for it

but rather the contrary; and the same may be said of a cup of tea; but when I sat down to dinner, and had eaten food about five minutes, the great sense of exhaustion, the distressing headach, the fluttering pulse, and the excess of alkalinity of the blood, began to pass away, and before the meal was over I was in perfect health. Hence it was again proved that physic, however well directed, is of little use to the starving man.

The other preliminary remark which I venture to offer is, that this low and uniform line, representing the respiratory changes which I have shown to occur during a lengthened fast, is precisely that to which the respiration falls at the end of the interval between meals; so that, whether with or without food, there is a tolerably uniform basal line below which the system does not pass—the elevation of the line differing at different periods of the year. Above this line there is an elevation from the influence of food—an elevation which is also tolerably uniform, and restricted to a maximum increase of from two to three grains of carbonic acid per minute. It is not the same after every kind of food, and with meals is the greatest after the breakfast and the tea. Thus there are two degrees in which food may be proved to act, both of which are distinct and well defined; viz., that which maintains the chemical changes to the minimum line, and that which raises them above it. It is, however, clear that the former does not suffice alone to maintain life, and therefore that the latter, although a temporary, is an essential action.

I shall next refer to the effects of several substances which act in one or both of the degrees now pointed out.

Pure starch as arrow-root, and fat in various forms, but chiefly at the present day as cod-oil, are extensively prescribed; the former to infants, and also to adults with an enfeebled state of the assimilative function, under the idea that it is highly nutritive and easy of digestion; and the latter, in cases of wasting and cachectic states of the system. My experiments have shown that, although they are respiratory foods, they do not excite the respiratory function; or, to speak more accurately, starch gives a very slight increase, and fat causes a decided decrease. The action does not exceed that minimum or basal line which I have just shown to be insufficient to maintain life.

must enter that fluid as free acid, and, until its transformations are completed, it will necessarily have an acid reaction. This would be exerted for a limited period only, as was shown by the effects described.

I have already stated that after a long fast, when the system must require food, arrow-root and water gave no feeling of satisfaction, but, on the contrary, a sense of sinking at the stomach and small intestines; and its effect upon the respiration was scarcely more than occurs in the entire absence of food. The pulse was reduced in frequency. Hence we find an explanation of the impropriety of attempting to feed infants on arrow-root boiled in water, under the idea that arrow-root alone is a nutritive food. It has been abundantly proved that animals cannot live on starch, and yet it is not uncommon to find mothers with deficient milk giving to their infants arrow-root, or some of the fashionable preparations of corn, which consist almost entirely of starch freed from the important nitrogenous constituents, and using water instead of cow's milk, under the impression that the latter would be too rich a food. Such a course can only lead to starvation.

But starch, apart from other substances, is not found in nature, and the result of its action is very different when it is administered in the combinations so universally provided for man, viz., in the cereals. These, whether wheat, oats, or rice, have a most powerful and enduring influence over the respiratory function—an influence extending to an increase of two grains or upwards of carbonic acid per minute, and continuing to the extent of upwards of one grain per minute for between two and three hours. With these foods, moreover, there is a complete sense of satisfaction, and it has been proved that they are capable of sustaining life. Hence the administration of wheat, flour, or bread, instead of arrow-root, where we desire to improve the nutrition, is clearly indicated. The uniformity of action of these three cereals, some of which are found in nearly all parts of the earth, and upon which the life of man universally depends, is an interesting fact, for in all essential respects their action is identical, the quantity being varied according to their relative variations in carbon and nitrogen. Rice, however, is chiefly used in hot climates, and tends more to excite pulsation; and oatmeal, which is almost exclusively employed in cold climates, appears to have the advantage of maintaining a tolerably uniform and elevated line of respiratory action for a more lengthened period than the others. Hence oatmeal is not only a good, but a cheap food, as our northern brethren know well.

There is also another fact of importance connected with the use of the cereals, viz., that however valuable more highly nitrogenous food, as beef or beef-tea, may be, they do not, when taken alone, at all equal in value wheaten bread; for, whilst

the former can at the most only sustain the waste of tissues, the latter is capable of sustaining every function of the body[a]. Hence, in cases of debility, and particularly of impending exhaustion, where the system has probably for a long time experienced more waste than supply, the too frequent habit of relying upon beef-tea and wine, to the almost total exclusion of the cereals, is not defensible. In all such cases, whilst we may seek to arrest waste by any remedy which is presumed to have that power, the real supply must be derived from the cereals. No one who has, when fasting, taken even *good* beef-tea alone can have failed to notice the very insufficient sense of satisfaction which it gives, and how soon there is a feeling of sinking or malaise about the stomach and small intestines; and it may be laid down as indisputable that a food which does not give a sense of satisfaction is insufficient to support all the processes of the system. I am desirous, therefore, of affirming the importance of the use of the cereals, with or without other food, as that by which waste must be repaired in cases of prolonged exhaustion, whether with or without imminent peril. In the opinion of Dr. Maclagan, expressed in a letter with which he has kindly favoured me, the use of beef-tea, freed from albuminous compounds, is simply to lessen waste; an important influence, no doubt, but as that substance does not supply the material which is required by the system, it only delays, and does not cure the evil. The cereals have a quick as well as a powerful influence; so that within half-an-hour they cause an increase of one grain of carbonic acid per minute; and in this respect they are not exceeded by any of the substances which are true foods.

The true influence and value of potato have, I think, been often misunderstood. I find that it differs from the cereals in the lessened duration rather than in the amount of influence, for with eight ounces of good potatoes I always obtained an increase of one and a half grains of carbonic acid per minute. The effect did not usually endure longer than one and a half hours, and the subsidence of it was very rapid. When

[a] If it be objected that there can be no proper comparison of beef-tea and the cereals, on the ground that the former does not possess plastic compounds, I venture to remark that it is far from being a settled question as to how far gelatinous compounds are useless in the formation of tissue, and that the well-known "Gelatine Commission" by no means settled this question. I have proved, as will be seen further on, that gelatine acts, like other nitrogenous compounds, as a respiratory excitant; and moreover, if beef-tea is properly prepared, it contains much albumen and plastic matter. If the imperfect knowledge which we at present possess is to be the standard by which all new investigations are measured, we can make no progress, and he who so uses it is an obstructor of knowledge.

conjoined with mineral matter, it has been abundantly proved to be capable of sustaining life, and hence it has not, I think, obtained that credit which is its due. There are, however, two disadvantages, in respect of one of which I think there is a common error, viz., that a large quantity must be taken in order to afford a sufficiency of nutriment for the respiratory function, and even then the quantity would be deficient in nitrogen; and in this quantity it is a much dearer article of food than the cereals. It is in this latter particular that popular error occurs, and one, moreover, which the dietaries of our prisons and work-houses show to have extended to scientific men too. To supply a given amount of carbon and nitrogen to the system, it costs two and a half times as much to obtain it from potato as from bread, and it cannot be too widely known that the cereals are not only the best but the cheapest of foods.

Milk, also a fundamental article of food, and one which enters largely into our sick dietaries, either as new milk, skimmed milk, or cream, appears to have different actions upon different individuals, if we may judge by the variety of tastes regarding it, and the ill effects which many ascribe to its use. My experiments show both that it has a powerful and enduring action, and that it differs in its physiological effects in different persons. In my own case (I am fond of milk) the effect of new milk is extremely analogous to that of the cereals, both in the extent and the duration of its action—a fact of great interest, since each is able to support life alone for very considerable periods—both being, in fact, alike the "staff of life" at different periods[a]. It is also of interest to know that the action of every element of the milk, viz., the sugar, acid, casein, and cream, tends in the same direction, but that no artificial combination of these elements, and no separate element, has the full effect of the natural new milk. New milk increases both the respiration and the pulsation, and in the latter effect it does not correspond with the cereals.

Skimmed milk is held in great repute amongst the German physicians; and in Greece, Turkey, and the western United States of America, sour milk, or milk with the casein coagulated, is a most common article of food. In skimmed milk we still retain the casein, acid, and sugar, and I have proved that it produces an increase of the respiratory function to the extent of more than one grain of carbonic acid per minute. Casein alone,

[a] In the higher mountainous regions of the Tyrol Switzerland, and Sweden, the mountaineers drink a gallon, or other very large quantity, of milk daily, and in the comparative absence of the cereals, this is in adult, as in infant life, their principal sustenance.

and sugar of milk alone, will each produce nearly the same effect; but lactic acid scarcely, in ordinary conditions of the system, affects the respiration. Hence, I believe the value of skimmed milk to be chiefly referable to the casein, and that not simply as a plastic material, but, as I shall presently show, from its power to increase the transformation of respiratory food, in fact, increasing the assimilation of food. On this ground only, I think, can we account for the fact to be presently noted; that the addition of one half pint of skimmed milk to the ordinary dietary of prisoners was of remarkable benefit. It is also highly probable that in states of the system in which there is an unusual tendency to an alkaline state of the blood, as, for example, in the hot season, the free use of the lactic acid in sour milk tends also largely to increase the transformation of material, and thereby to improve the assimilative function by causing a more rapid interchange of the acid and base in the blood, and the more rapid disengagement of carbonic acid. Thus, in cases in which we desire to avoid increase of pulsation, where we wish to increase the transformation of nutritive material without adding to the carbon in the system, and where we find an acid rather than an alkaline fluid to be indicated, skimmed milk, or sour milk, as the case may be, is preferable to new milk. Such, no doubt, are many cases of fevers. Cream, in days gone by, supplied the fat in sick dietaries which is now given in a more nauseous form, and in many cases with good reason, for it has a greater influence over the respiratory function than separated fats, since it retains a certain amount of acid and sugar, and it has an equal influence over pulsation.

Hence we find an explanation of the well-known fact of the great propriety of combining bread with milk in food when we desire to well nourish the system, and the proof that they are in truth the sustainers of animal existence.

I have already stated that fats do not increase the evolution of carbonic acid, but, on the contrary, that they lessen it. This is seen in both butter and olive-oil, the two kinds of oil or fat used most commonly as food; but it is most evident in cod-liver oil, the fat most commonly used in medicine. The depression is not, however, great. There is also a soothing effect when any considerable quantity is taken at once, as one ounce of butter or cod-oil alone, not very pronounced, but sufficient to be noticeable and pleasant. Fats always increase the pulsation, and in about twenty minutes cause a sense of satisfaction and of slight fulness at the heart. As fats are not transformed in the process of digestion, but pass into the lacteals in a molecular state, and are deposited in the

tissues as fat, it must be evident how advantageous a mode of administration we have in milk with its alkaline reaction, in which the oil is ready divided into molecules and associated with albuminous material ready to its use. Hence I think that new milk is a better mode of administration of fat than cream even, and much more than separated oil, provided a quantity be taken which will supply the required quantity of fat. Each pint of milk contains about three-quarters of an ounce of butter.

As fat and starch are similar aliments, and yet both seem to be necessary to the system and to be agreeable to the appetites of mankind, it is of the greatest interest to determine the mode by which they severally act, so as to ascertain the difference which must exist between them. No satisfactory explanation has been given; and, although I have ascertained several respects, both physically and physiologically, in which they differ, I feel that further observations are still necessary. With regard to the influence of temperature, I am assured by Sir James Ross that no large increase of fat is necessary to our sailors in the Arctic regions; and by Professor Sandhal, of Sweden, that the peasantry of Lapland do not take food materially differing from that of our own country. Moreover, in hot climates, as in Italy and China, fat is used largely with the starch, and hence I think that in practice there is less difference amongst mankind than is commonly supposed. We must also recollect that the flavour of fat is agreeable, and that its physical properties render the mastication and deglutition of bread more easy and agreeable; and, in fact, that fat, being taken with bread or flesh, becomes associated with substances which tend to call it into action. In reference to the action of fat when rubbed into the skin, and which has been often shown to be beneficial, I venture to suggest that the probable mode of action is chiefly by modifying the heat-dispersing process by that organ. Inunction of oil has ever been a luxury in hot climates, and particularly after the prolonged use of the hot bath—that is to say, in conditions in which the exhalation of vapour from the skin is exceedingly rapid—also where the direct rays of the sun have induced irritation in the skin. There are also many conditions of system in our own climate in which the skin is too active in relation to the amount of chemical change proceeding within; and hence, with less generation of heat, there is increased waste of it by this great refrigerating organ of the body; and, as a consequence, there is a continually increasing tendency to enervation. If in such cases we cannot increase the appetite, or by any other means increase the vital actions,

by lessening the waste of heat, and thereby the necessity for heat, we effect a positive, although not an equivalent good.

That a layer of oil suffices to close the pores of the skin for a time, and thereby lessens the transpiration of vapour, may be inferred from the action of it when laid upon the spiracles of an insect, or even upon the skin infested with parasites, such as the pediculari pubis, when it arrests respiration, and the creature dies. Moreover in man, if the skin be well coated with a thick fat which does not liquefy, the transpiration of vapour, and, *à fortiori*, the absorption of heat which occurs with evaporation, and which cools the body, are prevented. This effect will vary with the viscidity and quantity of the oil, the rapidity with which it is removed from the skin, and the frequency of its application. That some of the oil is absorbed and acts as a nutrient is possible; but when it is remembered with how much difficulty fats are absorbed by animal membranes, and by the capillaries in particular, and that this treatment is of use in certain cases only, it is not possible that this is its chief mode of action. The cases fitted for this treatment are those of debility with lessened appetite and feeble digestion, and with moist skin and relaxed tissues. Such are many cases of phthisis and many others recovering from severe and long-continued disease.

But starch and fat, whatever may be their transformations and modes of action, are, as already stated, almost exclusively restricted to that amount of action which will occur in the absence of food, and which, as has been shown, is insufficient to maintain life. When, however, they are associated with certain other substances, as they usually are in nature, they obtain the power to increase the respiratory action above this basis line. This class of substances is very extensive and important, and I have ventured to call them "respiratory excitants." Without some of them we could not live; and prominent amongst them are the well-known *nitrogenous foods*, which hitherto have been presumed to have no connexion with the respiratory process. The cereals contain three members of this class: two nitrogenous, viz., gluten and albumen; and the other non-nitrogenous, viz., sugar. Milk offers sugar as well as a fourth nitrogenous substance, viz., casein. *Casein* and *gluten*, when taken alone, increase the respiratory process to the extent of nearly one grain of carbonic acid per minute, the increase enduring upwards of one and a half hours. They are well known as powerful ferments. Gluten is at the present time a subject of dispute in reference to the value of flours, it being understood that "strong" flour, as it is termed, contains a larger

quantity of gluten, and that the wheat is, *ceteris paribus*, valuable for mixing in proportion to the yield of gluten. This is an important corroboration of the truthfulness of my inquiry into the effects of this substance, and shows that the excito-respiratory elements of wheat are essential to the due action of the starch. In my experiments, however, there was a maximum result obtainable, whatever, within limits, was the amount of gluten taken; and hence it appeared that gluten being present, the quantity of it received within a limited period is not very material. This fact seems to regard gluten as a ferment.

*Casein*, well washed from skimmed milk, takes the place in milk that gluten occupies in bread, and is an important member of the excito-respiratory class. This substance cannot be properly represented by cheese, since cheese contains much fat, and undergoes secondary changes which render it less easy of assimilation. But it is doubtless to the casein in cheese that must be ascribed the digestive quality, and for this purpose it is known that, whilst a small quantity may be beneficial, a large quantity is a cause of disturbance to the functions of digestion—a fact analogous to that just mentioned in reference to gluten.

*Sugar*, however, is the most powerful respiratory agent found in milk and cereals, and, of the different kinds, cane-sugar is the most powerful. The effect of one ounce of white sugar dissolved in water is to increase the respiration nearly one grain of carbonic acid per minute in less than eight minutes, and to produce a maximum increase of from one and a half to two and a half grains of carbonic acid per minute in twenty-five to forty minutes, after which period the effect rapidly subsides. The amount of influence is, however, probably in some degree dependent upon the state of the blood, and the rapidity of its absorption into the blood; for in some conditions the addition of an acid increased and shortened its action, whilst an alkali had the contrary effect. The importance, therefore, of sugar with our food is very great, and is in accordance with the appetites of persons of all nations and of all ages. It is probable that certain ill effects have been unduly exaggerated. The great desire for sugar and water on the continent is well explained by the gratifying coolness of the fresh-made beverage, its agreeable flavour and innocuousness, and also by the great freedom and ease of respiration which attends its immediate action. In many of these respects its action is very different from that of alcohol, so much used in this country. The effect upon the respiration is very marked; and, although the quantity of air inspired is sometimes increased so much as one

hundred cubic inches per minute, the great freedom of inspiration seems to indicate a still greater increase. The pulsation was commonly increased, and the pulse became fuller. There was a sense of satisfaction after it had been taken, but this was only for a short time; and after about an hour and a half there was craving for food. Hence it has been with some propriety called by the Germans a "Genuss mittel" and not a "Nahrungs mittel." It does not, in fact, supply the place of food. The component parts of flesh, as albumen, fibrine, and gelatine, are recommended with a view to increase the plastic material and tissues of the body; but whilst they undoubtedly have this power, they also possess an excito-respiratory action, and that in a degree but little inferior to casein and gluten.

*Fibrine*, as excellent lean beef-steak, has, however, less respiratory action than any of the others. The extreme increase in carbonic acid, after having taken albumen in the form of two eggs, was 1·12 grains per minute; while a maximum increase of between three-quarters and one grain per minute will represent the ordinary influence of gluten, casein, albumen, and gelatine.

Hence we arrive at the important fact that all the nitrogenous foods have, in addition to their power of forming protein compounds, or of otherwise sustaining the changes in the tissues, a most important action over the respiratory changes; and it is now proved that it is one thing to supply respiratory food, and another to call, or aid in calling, that food into action. In illustration of the importance of this class of foods, the following statement is very interesting. In 1853, the prisoners at the Wakefield Gaol lost weight so much that 22·35 per cent. were continually on extra diet. The surgeon, Mr. Milner, advised an addition of half-a-pint of skimmed milk to the general dietary for breakfast, after which the extra diet was reduced to fifteen per cent. in the following nine months, and to fourteen per cent. in 1856. This could not have been due to the amount of nutriment in the milk, but to the nitrogenous compounds acting indirectly. The importance, therefore, of nitrogenous food is thus rendered greater than has been hitherto regarded, and the indivisibility of the two classes of food, viz., respiratory and plastic, is rendered more evident than chemistry has of late years sought to prove. Physiology has ere this shown that, with the body at rest, the evolution of carbon by the respiration is attended with that of nitrogen in the excretions, and that increase of respiration was accompanied by a proportionate increase in the excretion of both carbon and nitrogen, and in such a manner that there was a close correspondence in

the increase of each. Both of these circumstances seem, therefore, to prove not only the indissolubility of the two processes, but that both occur at the same time and also in the same place, viz., in the tissues.

*Tea* is a substance which has hitherto been regarded as a nutrient[a], because it contains a little nitrogen in its active principle, and has a proportion of gluten in the leaf. Nothing can be clearer from my experiments than that it promotes chemico-vital action, and causes the evolution of more carbon than it supplies; and Lehmann has ere this proved that it increases the elimination of nitrogen. As this is a subject of great interest, I have investigated it in every direction; and, under all circumstances, in every proportion, and in every experiment, it has proved itself to be a powerful respiratory excitant. It is equally efficacious when taken cold, and after it has been prepared two days, as when hot and recently made. Green tea is somewhat more powerful than black; but both kinds, when strong, caused a temporary nausea, or, sometimes, narcotism. The addition of an acid, as is practised by the poorer inhabitants of China and by the Russians of all classes, renders it more stimulating, whilst the addition of an alkali makes it more soothing or narcotic. Both acids and alkalies rather lessen its power, and a fixed alkali totally destroys its influence. Acids, moreover, usually shorten the duration of its action, whilst an alkali prolongs it; but this varies, as the action of acids and alkalies taken alone varies with the existing state of the blood. The maximum increase in the carbonic acid was from one and a half to three grains per minute, and was obtained in twenty-six to fifty minutes; but an increase of a half to one grain per minute was found in ten minutes. The total duration of its influence is usually about two hours. The repetition of small doses, as twenty-five grains every quarter of an hour six times, produced an increase in the evolution of two hundred grains of carbonic acid, a quantity nearly four times that obtained when the whole quantity was taken at once. With this increase of the chemical changes there was an equal increase in the volume of air inspired, and a great increase in the depth of inspiration, accompanied by a sensation of lightness and clearness of the mind, and great ease and completeness in the respiratory effects—results which correspond with popular observation.

As the action of tea has not been understood, it has not

[a] See Johnson's Chemistry of Common Life.

obtained that place in medicine to which it is certainly entitled; and hence I am desirous to place its action prominently before the profession. A remedy which speedily and most powerfully increases the respiratory function, which has a rapid accumulative action, which can be kept for some days without injury, and taken either in a dilute or concentrated form, must be a good remedy in many diseases. Liebig has with great sagacity pointed out the correspondence between this substance and the active principle of the bile; and there can be no doubt that, with deficient action of the liver, with or without deficient action of the respiration, in relation to the nutritive material and effête matters remaining in the system, tea is a most suitable remedy. In cases of general debility and in phthisis, in both of which we have deficient respiratory action, it seems at first sight to be contra-indicated, because there is already in such cases deficient supply; but even then, when it is conjoined with nutriment, I believe it to be one of the most efficient agents which we possess. The same reasoning, viz., the balancing of waste and supply, will point out the cases in which this agent is not free from harm, so that it is fortunate, perhaps, that the poor woman, with her morsel of bread and her half-a-dozen cups of tea, is obliged to be content with a weak infusion, and a tea not very pure. A very striking illustration may be derived from an experiment made at the Wakefield Gaol, in which Mr. Milner gave one pint of tea for supper instead of one pint of gruel, containing two ounces of oatmeal, and he immediately found the prisoners to lose weight. He then added the tea to the gruel, and found that they still lost weight in a greater degree than with gruel alone. Hence, tea without excess of food may do positive injury. The powerful excito-respiratory action of tea would also probably contra-indicate its action in cases of inflammation of the respiratory organs. There are two other circumstances under which I venture strongly to recommend its use, viz., to soldiers on march in hot climates, and in cases of suspended animation. It has been abundantly proved that the use of spirituous liquors increases the evil effects of exposure to the sun, and that a principal source of danger arises from deficient action of the lungs, as is well pointed out by Mr. Ranald Martin, F. R. S., in his papers recently published in the Lancet, and particularly in a very valuable report just published by Mr. Longmore. Moreover, I have proved that with increase of temperature there is diminished respiration and increased pulsation. Tea appears to be very peculiarly fitted to avert the dangers if properly administered, since it not only excites the respiration,

but increases muscular energy, determines to the skin, and thereby cools the body, and that without increasing the pulsation. The proper mode of administration would be for the soldier to carry cold infusion with him, and to drink a due portion (say twenty-five grains) every half hour or hour during exposure.

The same objection to spirituous liquors, and the same advantage from tea, may be applied to cases of restoration from drowning; as an adjunct to other efforts, and the more so that we have no better remedy, it is highly desirable, although its action is not so instantaneous as could be wished. In all such cases the prime object is to restore the suspended respiration, since by it chiefly can the action of the heart be excited and maintained, and the chemical changes renewed.

*Coffee* has a similar but not quite so powerful an influence over the respiration as tea. It differs, however, from tea in commonly lessening the action of the skin, as shown by the dryness, heat, and pulsation, and increasing the action of the bowels, as proved by the tendency to diarrhœa which occurs from a large dose. Hence, in cases of debility where there is excessive action of the skin—a perspiring and soft skin—with the excessive loss of animal heat which accompanies it, coffee is clearly preferable to tea.

*Cocoa* and chicory are both respiratory excitants, but in a less degree than coffee; and hence, chicory can only in a modified sense be regarded as an adulteration of coffee.

*Coffee-leaves* have an action opposed to that of the fruit. They are extensively employed by the poor in the island of Sumatra, and from their cheapness efforts are being made to introduce them into use in Europe. Their action is, however, mildly narcotic, so that they lessen instead of increase the respiratory changes. When taken in doses of half an ounce, the maximum decrease in the carbonic acid was so much as 0·89 grains per minute in myself, and 1·42 grains per minute in another. The quantity of air inspired, and the rate of both respiration and pulsation, all fell. Hence it has the qualities of a valuable febrifuge medicine, and is applicable also to many cases of nervous irritability.

The subject of the action of *alcohols* is one of the greatest interest to the physician, and one which the present state of science has left in a very unsatisfactory state. The term has been made to include every article of food which contains alcohol, viz., all kinds of spirits, wines, and beers; and it is assumed that these various substances have a similar mode of action. Hence, Vierordt drank half a bottle of white wine

and placed the results to the credit of alcohol. Common experience is on this subject far in advance of science; for it is well known that not only has each of the classes just mentioned different effects, but that each member of each class has a special mode and degree of action. Further, various qualities of each of these substances are well recognised, so that our northern friends know well the difference between what is called good and bad whiskey, although each shall be equally strong, or, as is very common, the worst shall be the strongest. The gin-drinker and the rum-drinker are very different men, as are also the gin-drinker and the beer-drinker[a]. Moreover, in practice we do not prescribe each of these substances indiscriminately, much less do we ever think of administering seventeen per cent. of alcohol with water instead of giving wine, or six to ten per cent. of alcohol with water instead of beer. Experience has shown that the effect of these articles is not in proportion to the alcohol which they contain, although we have been, with regard to wines and spirits, too much inclined to refer the effect to the alcohol.

It is quite clear that the presence of alcohol in varying quantities is quite insufficient to give a common action to these substances, and hence it would be quite consonant with reason if it were ascertained that their influence over the vital changes of the system varied according to the one selected. Chemical analysis has not done much to elucidate this subject; but we know that wines and spirits contain soluble ethers, and that a good wine is known as much by its aroma or volatile constituents in quantity and quality as by its flavour. Wines also contain sugar, vegetable acids, and salts. Inferior spirits, as raw whiskey, for example, also contain fusel oil. Beers are known to contain sugar and gluten amongst other things, besides alcohol.

I have made these trite remarks because I have found that each member of these classes has a distinct action, and that each one must be studied alone. *Alcohol* does not lessen the respiratory changes, but, on the contrary, rather increases them; and when the quantity is sufficient to act upon the sensorium, the influence is chiefly a disturbing one. Notwithstanding that this result is not in accordance with received opinions, I have not

[a] I am informed that in the rum-producing country, Jamaica, it is the habit to drink old rum with water, as an ordinary and oft-repeated beverage, and that it is done with impunity; but, when brandy is substituted for rum, the effect is so different, and so much more injurious, that the change of habit is at once recognised by the acquaintances of the victim.

the least doubt of its truthfulness, for the experiments have invariably given the same results. The maximum increase varied from a half to one and a half grains of carbonic acid per minute, with a corresponding increase in the quantities of air inspired; but, before the experiment terminated, the whole increase was often lost. When a smaller quantity was taken, every fifteen minutes, the effect became ultimately greater, more uniform, and enduring. Böcker remarks that it lessened the appetite; and, consequently, there would be an indirect and secondary diminution in the vital processes. My experiments do not show how alcohol acts; but, from the rapidity of its action (often within five minutes) and the unusual variations in the returns from succeeding experiments, due evidently to a disturbing cause, it is highly probable that it acts, at least in part, through the nervous system, and hence may excite the respiratory function in an indirect manner. It was also proved in our experiments that it lessened the action of the muscles subject to volition, but increased in a certain degree those acting more or less independently of volition; as, for example, those of the heart (the most important effect, I think, of the action of alcohols), and the respiration—a difference not in relation to the structure of voluntary and involuntary muscular fibre, but to a partial suspension of the act of volition, and to contact of the alcohol with the walls of the heart. It is known that sexual intercourse is rendered less possible after having taken alcohol, even in small quantities; and the explanation is probably found in the foregoing paragraph. We also noticed two stages in the effect of all spirits when taken in quantity to affect the sensorium. In the first, there was heat with determination of blood to the skin, lessened volition, and perception of light, sound, and sensation, increased pulsation, jumping inspiration, and merriness of spirits. In the second, sudden sensation of cold, yawning or sighing, feeble respiration, returning consciousness, and taciturnity. There was lessened action of the skin and mucous membranes, and increased action of the kidneys; the former appearing chiefly in the first; the latter, in the second stage. The increase in the respiratory changes was chiefly in the first stage. The sudden sensation of cold; the returning consciousness, and the desire to urinate, occurred nearly simultaneously.

*Brandy* and *gin*, and particularly the latter, always lessened the respiratory changes. *Whiskey* commonly lessened them, but sometimes it increased them. *Rum* increased them almost always, and to a greater extent than alcohol alone. The nu-

merous experiments which I have made during the last two years, both on myself and others, establish this fact beyond any reasonable doubt.

The vapour of all spirits, when inhaled, decreases the quantity of carbonic acid, but increases the amount of water exhaled from the lungs. The vapour consists of alcohol and the volatile ethers. The vapour of fine old port-wine had the same influence, but in a greater degree. Sherry-wine, when drank in doses of three ounces, had no very marked influence, the effect being sometimes slightly increased, and at others decreased.

*Ale* and *stout* uniformly and invariably increased the respiratory changes, and maintained the increase for a lengthened period. Rum and milk had a very powerful and enduring influence in increasing the respiratory phenomena, and proved that their influence, as a restorative, is extremely great. The combination, moreover, prevented any effect upon the sensorium.

Hence I draw the following deductions:—

1st. Alcohol does not act principally upon the respiratory function; and when it lessens the vital changes, it does so, not by retarding waste, but by lessening supply. It is a disturber of the system, and cannot be regarded as a food.

2nd. Beers act upon the respiratory function probably by the saccharine and nitrogenous elements, and hence act after the manner of ferments.

3rd. Wine acts upon the respiration probably by its saccharine and acid elements, and increases the respiration in proportion to their quantity, whilst it decreases it in proportion to the excellence and quantity of their volatile elements. Fine old wine lessens the respiration more than a newer and inferior wine.

4th. Rum and ale are true restoratives; that is, they sustain and increase the vital powers; whilst brandy and gin act in a negative manner by lessening action. The old combination of rum and milk is the most powerful restorative with which I am acquainted, but it is less conservative than brandy.

5th. It is probable that the common action of all the spirits in diminishing the action of the skin, and thus lessening the waste of animal heat, has a conservative tendency; whilst their power in increasing the action of the heart is a chief cause of the temporary benefit obtained from them.

The very different action of tea, and such spirits as brandy and gin, is very striking, as the following abstract will show:—

| CERTAIN ALCOHOLS. | TEA. |
|---|---|
| Lessen respiratory action. | Largely increases respiratory action. |
| Lessen action of skin and mucous membranes. | Increases action of skin. |
| Increase action of kidneys. | |
| Increase pulsation. | Rather lessens pulsation. |
| Decrease consciousness and sensibility. | Increases consciousness, sensibility, and mental perception. |
| Decrease voluntary muscular action. | Increases all muscular action. |
| Are followed by reaction and a miserable feeling. | There is no reaction, and the lightness and cheerfulness continue. |
| Lessen the desire for the supply of food. | Does not lessen supply, but increases waste. |

Having thus described the influence of the most important articles of food, I shall conclude this communication with two or three general remarks and a short *resumé* of the principal points which have been indicated:—

1st. It is known that whilst there is a general correspondence amongst men in the food which they desire, there are many exceptions, both in the healthy and diseased system. In my inquiries I found that with a disrelish for an article of food there was less influence from it than under ordinary circumstances, so that, in reference to milk, the effect of every element was less in Mr. Moul (one of the subjects of experiment) than in myself, and neither he nor any member of his family can take milk or cheese. Hence, appetite for food is the expression, not only of desire, but of fitness. Moreover, it was found that in the same person the various substances which were disliked had a common mode of action; also, that with this disrelish there was an unusual enjoyment of some other article having a similar mode of action. Thus, one disliking milk and sugar was very fond of tea. It is, therefore, very questionable how far it is proper to induce a person to take that which he disrelishes (assuming that it is not mere wilfulness), and whether it is not better to seek out and remove the state of system causing the disrelish, and to administer another article of food of an analogous action. Thus, an important meaning is given to that which is commonly regarded as irrational or capricious.

2nd. The effect of small quantities of a substance often repeated is much greater than when the whole is given at once. This was found in both tea and alcohols. Twenty-five grains

of tea, given every quarter of an hour, produced more than three times the effect of one hundred and fifty grains taken at once; and half an ounce of rum, or alcohol, taken every half hour, produced a later but a much more lasting influence. The value of small doses of medicine has been appreciated of late years, but the frequent repetition of them is not sufficiently understood. Hence, one ounce or two ounces of wine are given in an important case at intervals of several, and sometimes of many hours, whereas the influence of the wine begins in a few minutes, obtains its maximum in less than one hour, and soon afterwards disappears or manifests its secondary influence. If a small quantity were given every quarter or half-hour, the stimulating effects would be greater ultimately, and it would be longer sustained. In a case of imminent peril from exhaustion with fever, but with the skin soft, I gave, to a female patient, one wine-glass of port-wine so frequently that six bottles were taken in forty-eight hours, bringing the pulse down progressively from one hundred and fifty to ninety per minute. In such cases the interval should not be greater than half-an-hour.

The importance of the class of "respiratory excitants" having been pointed out, and, in a paper published in the Medico-Chirurgical Transactions of 1859, the cyclical variations of the respiratory phenomena having been demonstrated, it is of importance to determine what relation exists between them, viz., whether "respiratory excitants" are more demanded at the season when the respiratory phenomena are the least active. This also applies to climate, and is essentially connected with the influence of the two most powerful members of the class, viz., exertion and cold. It is clear that, in proportion to the amount of starch or other material to be transformed, so must be the necessity for the action of some of the respiratory excitants; and also that in hot seasons the influence of the two most powerful, viz., cold and exertion, is lessened whilst there is a diminution of the respiratory changes. Hence it is highly probable that in the hot season the free administration of certain of the azotized excitants is more necessary than is believed when the amount of carbonaceous food is not materially lessened. The free use of tea and coffee in hot climates is connected with the same requirement; and the necessity for animal food, which is said to occur in prisons during a long imprisonment, is explained by the necessity for respiratory excitants, and not for the plastic action of the nitrogenous food—there being an unusual absence of exertion, and therefore of waste. The diseases of the hot season are those indicating

lessened chemical change, whilst those of cold seasons are such as are commonly associated with highly nitrogenized food and hyperoxygenation[a].

The following are some of the principal points to which I wish to draw attention:—

The impropriety of administering arrow-root, or any form of pure starch alone from its deficiency in excito-respiratory material.

The great advantage of supplying oil as it is combined in milk, cream, and flesh.

The imperative necessity for the use of the cereals or milk when we wish to *sustain and increase* the vital powers. The *conservative* action, or *tendency to lessen waste*, of beef-tea and brandy.

The very diverse modes of action, in these respects, of rum and milk on the one hand, and brandy and beef-tea on the other, and the necessity for discrimination as to the action which we desire.

The respiratory as well as plastic action of all animal and nitrogenous substances.

The powerfully stimulating action of tea, both for good and for evil, as the case may be, over all the vital processes of the system, and particularly over those of the skin and lungs. The distinction between tea and coffee in their action upon the skin, and therefore upon the heat-dispersing process.

The very diverse action of brandy and gin from tea, but the nearer approach to similarity with coffee in consequence of their action upon the skin.

The very marked advantage of small and often repeated doses of substances which control nutrition but do not supply nutriment, as tea and alcohol, over large and isolated doses.

The indications of dislikes of foods.

[a] See a paper "On some of the Cyclical Changes connected with Season," in the Medico-Chirurgical Transactions for 1859.

Art VI.—*Remarks on a Critique of Dr. Brinton's on my Memoir on a little known Function of the Pancreas.* By Lucien Corvisart, M.D., Paris[a].

Dr. Brinton has done me the honour to submit to the test of his own experiments[b] my researches[c] on the pancreas, or rather some points in these researches.

Dr. Brinton's work would have had more weight if the author had not shrunk from repeating the vivisections from which I have derived so much information; and, above all, if, before undertaking or recording his researches, he had been more fully acquainted with the medical literature of the subject.

In fact, at the period at which Dr. Brinton published his observations, Professor Meissner[d] had, under the title of "Investigations as to the Digestion of Albuminous Bodies"[e], completely confirmed the fact of the energetic digestive action of the pancreas on albuminous aliments, which it would have been well to have repeated

Moreover, I had pointed out, in a short notice[f], the gross error which had misled MM. Keferstein and Halwachs.

It is well known that Montègre, in studying the gastric secretion, *always sought in vain* for the gastric juice, because he always sought it in the fasting state, and that by this obstinate course he was led to deny the existence of the gastric juice, which, nevertheless, thrust itself forward before the eyes of all.

MM. Keferstein and Halwachs, my opponents, falling into an error of the same kind, had rendered themselves powerless, and had, so to speak, concealed from themselves the action of the pancreas, by forgetting that it is not during fasting and repose that the gland must be taken, to find in it its ferment and its activity, but during the very act of digestion (five or six hours after a meal in the dog).

But if I thus dwell upon publications anterior to that of Dr. Brinton, it is because he, as well as Montègre and MM. Keferstein and Halwachs, has, without knowing it, very evidently divested most of his observations of any force.

I had taken some pains to spare experimenters the annoy-

[a] Translated for M. Corvisart, from his Manuscript, by William Daniel Moore, M.B., M.R.I.A.

[b] See the Number of this Journal for August, 1859.

[c] Sur une fonction peu connue du Pancréas, etc., par Lucien Corvisart. Paris: Victor Masson. 8vo, pp. 123.

[d] Zeitschrift, f. rat. Med. 1859, dritte Reihe, Band viii.

[e] Untersuchungen über die Verdauung der Eiweisskörper.

[f] The Lancet, June 18, 1859, and Union Médicale, 23rd July, 1859.

ance of the troublesome groping through which I had to pass, and to enable them to avoid negative or doubtful facts like those of Dr. Brinton.

"It is necessary," I said[a], "in preparing an infusion of the pancreas for the purposes of study, to avoid *crushing* the gland, or agitating it too frequently in the water, or protracting the infusion beyond the period when the fluid becomes clouded."

I had even taken care to point out this state as an indication for rejecting beforehand infusions which might have attained to opalescence, as inert, and unfit for the search after positive results. "Commonly," I added, "an infusion which is long filtering, and is cloudy, has partly lost its efficacy. . . . . The turbidity shows that the pancreatic juice is beginning to act upon the fatty matters in the gland itself; at a later period it will have begun to act upon the azotized substance, since, like the gastric juice, the pancreatic fluid exhausts itself by agitation"[b]. Taken in this state, the infusion of the pancreas would no longer exhibit to the experimenter any digestive action[c]. Now these very unfavourable conditions, which I have pointed out in order that they might be avoided, Dr. Brinton has selected and observed, and, what is more, he congratulates himself on having done so!

"The method I generally adopt is, to mince a given weight of pancreas very finely, before pounding it to a pulp in a porcelain mortar. Twice or thrice as much distilled water is then gradually added to and rubbed down with the pulpy mass; the whole is then filtered, so as to yield an opalescent (almost milky-looking) fluid" [?] "This process is in many respects superior to that of Corvisart" (Brinton). Thus we see the application of the adage "le mieux est quelquefois l'ennemi du bien." The process followed by Dr. Brinton, and which, before him, I had pointed out, not as a process, but as a mischance, is therefore fit only to show that it is possible in an instant, either in an entire pancreas, or in half a pancreas, to exhaust all digestive power on the albuminoids, *by pounding it;* which complete or partial exhaustion is recognised even

[a] Lancet, June 18, 1859, page 607. [b] Lancet, *loc. cit.*

[c] Facts are no doubt to be found in Canstatt for this year, due to Professors V. and S., which are completely confirmatory of another action described by me (Lancet, *loc. cit.*) and which will much surprise physiologists: when the pancreas is taken from a dog five hours after a meal, the juice contained in it is so energetic, that if the infusion of the gland be a little too prolonged, if the latter have been cut up in very small pieces, and, above all, if it be too often and violently agitated, the temperature being maintained at 104° F., the gland in part disappears, dissolved and digested in its own juice, which has escaped from the channels where normally it is confined during life.

before the digestive experiment, by the fact, that the liquid is milky and filters persistently turbid, facts which I had expressly stated, as has been seen.

The precaution I adopted of taking the pancreas immediately after the expiration of the fifth hour after a meal, of avoiding before and after death bruising or pounding the gland, or protracting the infusion beyond a period easily determined by observation, had led me to the discovery of facts invariably as similar as physiological facts usually are. It will be observed that Dr. Brinton's unfortunate deviation from this mode has led him, on the contrary, to fluctuate incessantly between my views and those of my opponents. At one time Dr. Brinton writes:—"Sometimes we may see no solution at all;" at another, he declares that "a rapid, energetic solution" occasionally occurs. In one place he writes:—"The solution is accompanied with unmistakable putrefaction;" in another, on the contrary, he says, "that an energetic solution is often seen to occur when, as yet, none but a peculiar, penetrating, quasi-syrupy odour is discernible." Now he declares, with me, that the solution is "rapid;" again he considers it to be "slow." Such are the uncertainties Dr. Brinton would have been spared, if he had been acquainted with the publications which have preceded his experiments.

Dr. Brinton seems, moreover, in the second place, to have made the same mistake as MM. Keferstein and Halwachs, in not choosing for his experiments the pancreas taken after the fifth hour after a meal.

Lastly, I may in the third place add, that although Dr. Brinton appears to trace a certain connexion between putrefaction and digestion, all the observations he has made, and in which some putrefactive odour, however slight, was developed, will never be considered by me, nor, I think, by any physiologist, as cases of digestion. I would particularly reject the experiment where, having proved the digestive inefficacy of a pancreas, he allows the fluid to putrefy during five whole days, then places it in contact with albumen, and pronounces that there was subsequent solution, and that the digestive power was increased by the putrefaction. The experimenter should always stop before there is any odour or possibility of putrefaction[a].

[a] The pancreas is one of the organs most liable to putrefaction, and this is an additional reason for vigilance in making experiments on the digestive action of its juice or of its infusion. At the temperature of 104° F., the digestion of fibrin is usually accomplished without putrefaction in the third hour; that of albumen in the eighth,

In this latter fact I see only two things:—

1. In the first instance, the pancreas, taken doubtless in the fasting state (nothing is specified on this capital point), is inert.

2. It was allowed to putrefy, and then communicated its putrescent liquefaction to the surrounding matters.

To recapitulate:—

1. Let Dr. Brinton never protract his experiments on artificial digestion beyond the period when the odour is the odour *sui generis* of the pancreas taken at the very moment of killing the animal.

2. When he meets with an inert pancreas, let him not seek in putrefaction an aid to solution.

3. Let him never crush the pancreas.

4. Let him reject beforehand as inert any infusion which passes through the filter turbid and lactescent.

5. Let him never take a pancreas except at the time I have thought it right to indicate.

6. Let him prefer the pancreas of a carnivorous animal, like the dog, where the periods of digestion are well marked.

7. Let him repeat the vivisections which he has very gratuitously refused to do, and, without referring to the confirmatory experiments of Professor Meissner, or those witnessed by MM. Kühne, Snellen, Professor Moris, Schiff, &c., I have more than a hope that, disengaged from a false starting-point, the experiments instituted by Dr. Brinton's able and methodical intellect will quickly and decidedly convince him as to the point in my memoir which has appeared to him interesting. Moreover, I earnestly invite experimental criticism of the others; whether they be regarded with favour or disfavour, they are closely connected with the physiology of digestion, and from the courteous collision of opinions, however evoked, the truth will be elicited.

if the pancreas has been taken out, and the infusion made within the preceding four hours. One hour's infusion at 104° F. suffices to remove from the gland all its juice. If the pancreas and its infusion be older, or if the digestion be more protracted, the experimenter is at fault, and allows the almost fatal supervention of putrefaction. Any infusion of the pancreas which requires more than an hour's digestion at 104° F. ought to be considered as inert.

Art. VII.—*Observations on the Mortality of Infants born in Workhouses.* By H. Minchin, M. B., F. R. C. S. I., Physician to the Hospital of the North Dublin Union; Lecturer on Anatomy, Steevens' Hospital School of Medicine, &c.

[Read at a Meeting of the Obstetric Society, December 3, 1859.]

Within the last few years the pens of several able and experienced medical observers have been engaged upon the important subject of the prevalence of disease and mortality in the early periods of life; and the more the subject is inquired into, the more does the interest attaching to the inquiry appear to increase. But, notwithstanding that the general bearings of this question, as far as statistical data have been attainable for the purpose, have already obtained a considerable amount of attention, and have been placed in a strong light before the profession, it would appear that much remains to be done in the way of exploring the several social subdivisions into which a matter of this kind naturally resolves itself. "So serious," says Dr. West[a], "are their diseases, that one child in five dies within a year after birth, and one in three before the completion of the fifth year." These figures have reference to the infant portion of the population at large throughout England, as deduced from the tables of the Registrar-General. We cannot, however, err very far in assuming that a similar rate of mortality prevails in our own part of the kingdom; but the uniformity of the proportion between the deaths of infants and the total number of births being liable to disturbance from a great variety of influences, this proportion, exhibiting probably a considerable fluctuation according to the habits, resources, means of subsistence, as well as the locality, occupation, climate, and a number of other social and sanitary contingencies affecting the several classes of persons among whom the births take place, there is no doubt that, by adopting a judicious division of labour, in investigating the causes of the comparative viability of infants in the different conditions of life, a valuable amount of statistical information would in a short time be accumulated, and much useful matter be brought together in reference to the important question—"Quare mors immatura vagatur[b]?"

[a] Lectures on Diseases of Infancy and Childhood, page 1.
[b] Lucret., v. 221.

The number of infants born in the 163 union workhouses of Ireland, during the year ending September 29, 1857, was 2369, and the number during the succeeding year, 2454. In reference to the mortality among this class, an unusual degree of interest began to be excited about the early part of last summer, and the popular feeling on the subject has been prevented subsiding altogether by the occasional appearance of a fresh article in some of the Journals on the "wholesale infanticide" which some newspaper writers have conceived to be in systematic operation throughout our land; the publication of the results of the Cork Union Inquiry appearing to have suggested the theme.

The question as to whether the infantile mortality which affects those born within the workhouses observes a higher ratio than that which obtains among *a similar class in the homes of the destitute*, is doubtless one which well deserves the attentive consideration alike of physicians and philanthropists; for if, upon due inquiry, the reply be in the affirmative, then indeed is some modification of infant management called for, and upon the medical officers of the above-named institutions would appear to devolve the moral responsibility of determining the defects, and suggesting the sanitary improvements. I am disposed, however, to believe that when all the peculiarities which characterize the condition of those children who have entered upon existence within the walls of the workhouse are fairly considered, their chance of survival will, perhaps, be found considerably greater than that of an equal number of children of like parentage outside the house. In the workhouse of the North Union, the infants born in the house constitute but a small proportion of the entire occupants of the nursery, the average ratio of births to admissions, during the last five years, being as 17·5 to 82·5 in every 100. In the present communication, however, I shall omit the consideration of the latter class, and limit my observations to a concise report of the results of 218 deliveries which took place in the Hospital of the North Dublin Union from 31st March, 1857 (the period at which I received charge of the Obstetric department), to the 30th September, 1859, with reference especially to the viability of the infants. An accurate registry is kept in the Hospital, in which are inserted the particulars of every labour that takes place, together with any circumstances worthy of observation in the subsequent history of the cases. I have been thus enabled to collect together some facts and figures, which, perhaps, may possess some interest

with reference to the question of the alleged excess of mortality among workhouse children.

During the period above specified, the number of obstetric cases admitted into hospital was 225; of these, 1 left the house undelivered; 6 were cases of hemorrhage in the earlier months, attended with abortion; and the remaining 218 were cases of delivery from the sixth month to the full term—3 of these being cases of twin birth; the number of infants born, therefore, was 221.

Of these 221 children, 183 were born living, and 83 were still-born, the proportion of the latter to the total being as 17·15 to 100, or 1 in every 6, very nearly; on comparing these numbers with the proportions stated in the reports of some of the principal lying-in charities of the kingdom, the ratio of still-births herein observed will doubtless appear very large. It should be stated, therefore, that, of the 38 children, 13 were prematurely born, and 8 of these 13 were putrid; while, of the remaining 25 who had arrived at full term, 13 were putrid. Thus it appears that 21, or more than one-half, were putrid, and 13, or more than one-third, were premature. Of the 38, the number of cranial presentations was 27; 5 presented with the nates; 2 with the feet; 1 with the arm; 2 were complicated with prolapse of the funis umbilicalis; and 1 presented the hand along with the head.

Of those labours which produced living infants, 169 were purely natural, according to the commonly received definition. The average duration was between seven and eight hours. 70 were primiparæ; 45 were second confinements; 19 were third; 13 were fourth; 8 were fifth; 4 were sixth; 2 were seventh; 3 were eighth; 3 were ninth; and 2 were tenth confinements. In each of the 70 primiparous cases, the average duration of labour was about ten hours.

Besides these 169, there were 10 cases in which labour was either tedious or complicated. To these are to be added 4 cases in which the nates or lower extremities presented; making a total of 183 children born alive.

Of these infants, 125 remained in the house for periods varying from a few days to fourteen months, and were ultimately taken out by their mothers, every one of them being in a good state of health; 18 remained in the house at the close of the period included in this report; and 40 died in the house.

The several periods at which death took place in the 40 infants are as hereunder stated:—

| | | | | |
|---|---|---|---|---|
| Within | 1 | month | 16 | died. |
| ,, | 2 | months | 7 | ,, |
| ,, | 3 | ,, | 1 | ,, |
| ,, | 4 | ,, | 2 | ,, |
| ,, | 5 | ,, | 2 | ,, |
| ,, | 6 | ,, | 4 | ,, |
| ,, | 8 | ,, | 2 | ,, |
| ,, | 9 | ,, | 1 | ,, |
| ,, | 10 | ,, | 1 | ,, |
| ,, | 11 | ,, | 2 | ,, |
| ,, | 13 | ,, | 1 | ,, |
| ,, | 15 | ,, | 1 | ,, |
| | | | 40 | = 21·85 per 100. |

The fact that two-fifths of the entire number of deaths occurred within the first month after birth is worthy of notice, corresponding, as it does, pretty nearly with what is observed among the infants of the general population; many of these deaths take place within the first few days or hours, and are manifestly referable to a congenital delicacy of constitution, although, from the circumstance of their being frequently attended with convulsions, they are generally attributed to the latter cause, primarily or solely. It should be remembered that a large majority of those born in the house are the children of women who have been confessedly living irregular lives previously to their admission, and who have availed themselves of the shelter of the house in many instances not until after their labour has commenced. Most of these unfortunate women are but ill adapted to discharge the functions of a mother, while their infants are usually delicate, stunted, or diseased, and a large number of them die before the mother's convalescence from parturition is established. That this early mortality is not to be attributed to any supposed unwholesome condition of the obstetric wards of the hospital may be inferred from the fact that the house has been remarkably free from puerperal disease for many years, only two cases of peritoneal fever having occurred during the entire period comprised in this report. A very good criterion, moreover, by which one may judge of the favourable sanitary state of these wards, is the almost complete immunity from infantile ophthalmia which has been enjoyed therein, even during those seasons when the elder children in the house have been visited most severely by ophthalmic disease.

Subjoined is a tabular arrangement of the 40 cases that died within the period already specified. It will be seen that 23

were cases of *first* children, and that in 20 of these the mothers were unmarried; 7 were second children; 4 were third; 4 fourth; 1 fifth; and 1 sixth:—

| No. of Cases. | Mother's Age. | Married or Single. | No. of Confinement. | Sex of Infant. | Cause of Infant's Death. | Age. |
|---|---|---|---|---|---|---|
| 1 | 30 | S | 3 | m | Syphilis. | 4 weeks. |
| 2 | 24 | S | 2 | f | Purpura. | 3 ,, |
| 3 | 20 | S | 1 | f | Measles. | 13 months. |
| 4 | 28 | M | 5 | f | Pemphigus. | 4 ,, |
| 5 | 30 | S | 2 | f | Measles. | 11 ,, |
| 6 | 22 | S | 1 | f | Bronchitis. | 5 ,, |
| 7 | 30 | M | 4 | f | Tuberculosis. | 11 ,, |
| 8 | 38 | M | 6 | f | Do. | 15 ,, |
| 9 | 38 | M | 3 | m | Atrophy. | 8 ,, |
| 10 | 22 | S | 1 | f | Syphilis. | 2 ,, |
| 11 | 26 | S | 1 | m | Atrophy. | 3 ,, |
| 12 | 20 | S | 1 | m | Syphilis. | 2 ,, |
| 13 | 24 | M | 1 | m | Convulsion. | 6 ,, |
| 14 | 25 | M | 2 | f | Do. | 7 days. |
| 15 | 25 | S | 1 | m | Atrophy. | 1 month. |
| 16 | 30 | M | 2 | m | Convulsion. | 8 days. |
| 17 | 24 | S | 2 | f | Do. | 12 ,, |
| 18 | 20 | S | 1 | m | Measles. | 5 months. |
| 19 | 23 | M | 1 | m | Convulsion. | 7 days. |
| 20 | 25 | S | 1 | m | Atrophy. | 2 months. |
| 21 | 18 | S | 1 | m | Do. | 4 weeks. |
| 22 | 21 | S | 1 | f | Convulsion. | 1 month. |
| 23 | 24 | S | 1 | f | Cholera. | 6 ,, |
| 24 | 34 | M | 4 | f | Hydrocephalus. | 9 ,, |
| 25 | 22 | S | 1 | m | Syphilis. | 2 ,, |
| 26 | 20 | S | 1 | m | Atrophy. | 6 weeks. |
| 27 | 25 | S | 1 | m | Convulsion. | 4 months. |
| 28 | 30 | M | 3 | m | Do. | 10 ,, |
| 29 | 25 | M | 4 | f | Atrophy. | 6 ,, |
| 30 | 30 | M | 4 | f | Cyanosis. | 1 day. |
| 31 | 27 | M | 2 | m | Convulsion. | 6 months. |
| 32 | 18 | S | 1 | f | Bronchitis. | 1 ,, |
| 33 | 20 | S | 1 | f | Cancrum oris. | 8 ,, |
| 34 | 23 | S | 1 | f | Convulsion. | 6 weeks. |
| 35 | 23 | M | 2 | m | Syphilis. | 5 ,, |
| 36 | 22 | M | 1 | m | Convulsion. | 1 month. |
| 37 | 40 | S | 1 | m | Do. | 2 days. |
| 38 | 23 | S | 1 | f | Do. | 2 ,, |
| 39 | 24 | M | 3 | f | Do. | 18 ,, |
| 40 | 20 | S | 1 | f | Do. | 6 hours. |

It may be necessary to make a few brief remarks upon some of the cases in the foregoing table. These will be referred to by the numbers in the first column.

Case II.—A face presentation; infant suffered from congestion; took erysipelas on the fifteenth day, and purpura on the eighteenth.

Case IV.—Mother far advanced in phthisis; infant delicate from birth.

Case V.—Delicate from birth; had been suffering for some time from the irritation of teething, when measles supervened.

Case VII.—Mother phthisical; infant delicate from birth.

Case VIII.—Delicate from birth; mother died of phthisis.

Case IX.—Very small and delicate from time of birth.

Case XI.—Prematurely born.

Case XII.—Mother had been under treatment for primary syphilis about two months before her confinement. The presentation was natural, but there was considerable delay in the first stage; the labour lasted twenty-seven hours, and the infant was apparently healthy at birth. In about a month syphilitic symptoms made their appearance, first about the anus and scrotum. Died in the second month, the mother's milk having completely failed for more than a fortnight before the child's death.

Case XIII.—Cachectic from birth; mother had suffered from syphilis.

Case XVI.—Mother a strong, healthy woman; twin case; the first was a footling presentation, a small and delicate child; died on the eighth day; the second a strong, sound female, presented naturally, and was born one hour after the first. There was but one placenta. Mother and infant remained in the house for thirteen months.

Case XVII.—Mother very delicate; second confinement. Labour lasted but six hours. The infant presented naturally, and was of small size; it took convulsions on the second day, and was relieved by castor-oil and a warm bath. Convulsions recurred on the fifth day, and were again relieved. It died suddenly on the 12th day.

Case XVIII.—Mother had syphilis; infant born syphilitic, but recovered under treatment. Died afterwards of measles.

Case XIX.—Mother healthy, primipara; labour natural, and of six hours' duration; infant appeared to be not more than of eight months' development; it had a very large *spina bifida* over the sacrum.

Case XXIII.—Very delicate from the period of birth; mother labouring under chronic hepatitis.

Case XXIV.—Mother aged 34; fourth confinement. All her former children dead. In this case the breech presented, and the labour proceeded very well till it came to the extrac-

tion of the head; there was a delay of several minutes, owing to the large size of this part, and the infant was nearly lost; ultimately, however, we succeeded in inducing respiration, and the mother and child were discharged on the eighth day. This child enjoyed good health till the seventh month.

Case XXVI.—Mother very delicate, and quite unable to suckle.

Case XXIX.—Very badly nursed, and very unkindly treated by its mother.

Case XXX.—Mother, aged 30; fourth confinement; labour short and easy. Infant appeared to be of about eight months' growth; very small and delicate. Respiration was very imperfect, and it died in fifteen hours.

Case XXXII.—Very delicate from period of birth.

Case XXXIII.—Badly nursed; mother very delicate; lacteal secretion scanty.

Case XXXIV.—Mother aged 23, primipara. In this case there had been several smart gushes of hemorrhage before the patient came into hospital. On examination, the os was found thin and dilatable, membranes entire; edge of placenta attached to the border of the os. On rupturing the membranes, the cranium pressed against the edges of the opening, and stayed the hemorrhage. Labour then proceeded naturally, and in about eight hours the child was expelled. Uterine action then ceased. Placenta retained for twelve hours, notwithstanding the employment of friction, compression, &c. It was then extracted, and the cause of its retention ascertained to be a morbid adhesion. Hemorrhage again in about two hours after the completion of labour; checked by the usual means. Patient recovered without any unfavourable symptoms, save that the lacteal secretion never made its appearance. The infant, which was originally healthy, died probably from unskilful or injudicious feeding.

Case XXXV.—Mother a delicate woman; second confinement; infant also delicate. About the eighth day a suspicious-looking eruption appeared on the mother; both nipples also became sore, and a circumscribed abscess formed in the left breast. The milk receded completely from both in the third week, and about the same time the infant began to exhibit symptoms of syphilis about the mouth and the anus; it died in a fortnight afterwards.

Case XXXVI.—A premature child; the mother had suffered from syphilis.

Case XXXVII.—A twin. The mother, a very delicate woman, aged 40, primipara. Both infants presented naturally

The first was of small size, and very delicate; it lived but forty-eight hours. The second was of good development, but putrid. But one placenta. There was no secretion of milk. This woman had all the appearance of syphilitic cachexia, complained of nocturnal pains in the bones, and her hair was falling out in large quantity. Although she would not confess to this cause of ill health, she improved rapidly upon bark with iodide of potassium.

Case XXXVIII.—Congenital delicacy; unable to suck.

Case XL. The mother of this child came into hospital three weeks before her confinement, labouring under anasarca, with albuminuria; improved very much under treatment; the renal secretion, when examined on the day before labour set in, was found to be free from all trace of albumen. In this case the labour was natural, and lasted eight hours. The infant was very small and delicate.

The cases of congenital malformation were but few: one of varus, a twin male, at present living and healthy, aged six months: one of double varus, a male, now ten months old, and quite healthy: one of spina bifida, of which I have already spoken. To these may be added one of inguinal hernia, which, as it presented an anomaly not often met, is entitled to a further description:—A young woman, who had been confined in the house about five weeks previously, requested me to examine the infant, which was *a female*, as she had perceived, on the previous evening, a small tumour in the pubic region of the child. On examination there was a visible prominence, just above the commissure of the labia, towards the right side; and beneath the skin could be felt a smooth, firm, elastic tumour, which increased in size during the efforts and cries of the patient, becoming then more prominent, and extending downward in the direction of the right labium. A distinct gurgling noise accompanied the reduction of the tumour, which was effected without difficulty. The point of the finger applied to the ring, could feel that this opening was much enlarged, the boundaries being very distinctly marked. The child's mother, being an intelligent young woman, stated that she perceived the tumour first on the day before she showed it to me; it is probable, however, that the condition of the parts was congenital. In other respects the child was quite healthy and well formed.

She left the house in a few days afterwards, and in about fifteen months she returned for the purpose of having something done for the child, the tumour having become much larger. The child at that time being strong, and able to walk

about, a truss was applied, and the case was lost sight of shortly afterwards.

The only case of this kind, of which I have been able to find a record, is one related by M. Billard[a]. It was the case of a female infant, seventeen days old, in whom a rounded tumour existed in the left inguinal region; the tumour was about the size of a hazel-nut, somewhat hard to the touch, *incapable of reduction* within the abdomen by the taxis, but neither diminishing by pressure, nor increasing during the cries of the infant. M. Billard, although at first disposed to believe this to be a congenital inguinal hernia, was cautious enough to decline giving a positive opinion to that effect, alleging that "*the sex of the child forbade the supposition.*" However, in less than four weeks afterwards, the child died of pneumonia; and on dissection the tumour was found to consist of the left ovarium, which had descended through the canal and inguinal ring; these parts having been much longer than in ordinary cases of females.

The very favourable sanitary condition of the obstetric wards, as evinced by the absence of any tendency to zymotic disease therein, has been already alluded to. During the entire period there were but two cases of serious illness among the mothers, and both were cases of peritoneal fever.

In the first case, the patient, aged 27, primipara, unmarried, had been in delicate health for a long time before admission, having had syphilis more than once. The fœtus was putrescent, and the placenta, which was morbidly adherent, required the introduction of the hand for its removal. Peritoneal symptoms set in on the fourth day, and were treated unsuccessfully. In the next case, which occurred seventeen months afterwards, the patient was thirty-six years of age, unmarried; second confinement. The labour was easy and natural, of about four hours' duration. The fœtus, a male of about eight months, was putrid. In her former confinement, about seven years previously, the child was still-born, but at full term. Symptoms of peritonitis were observed on the third day. The treatment in the beginning consisted in leeches, followed by bran stupes, with calomel, James' powder, and ipecacuanha, internally; also, mercurial inunction. On the fifth day, the system being fully under the influence of the mercury, this was omitted, and half-grain doses of opium every third hour were given instead. There was a marked improvement in all the symptoms on the sixth day, from

[a] Maladies des Enfans, p. 456.

which period the convalescence, although slow, proceeded satisfactorily. The strength was supported throughout by arrow-root, beef-tea, and wine; and she left her bed on the nineteenth day. In less than a fortnight afterwards she had a severe attack of asthenic pneumonia, for which she was treated successfully, with large doses of sulphate of quinia. She left the house quite well.

ABSTRACT.

Number of obstetric cases admitted in two years and a half = 225.

| | | |
|---|---|---|
| Abortions in early months, . . . . . | 6 | = 225. |
| Left the house undelivered, . . . . . | 1 | |
| Deliveries at or near full term, . . . . | 218 | |

| | | |
|---|---|---|
| 215 gave birth to single children, . . . | 215 | |
| 3 ,, twins, . . . . . | 6 | |
| 218 women. | 221 children. | |

| | Males. | Females. | | |
|---|---|---|---|---|
| Children born alive, . . | 103 | + 80 = | 183 | = 221. |
| ,, still-born, . . . | 24 | + 14 = | 38 | |

Proportion of still-born to total = 17·15 to 100.

Of the 38 still-born, 25 were at full term; 13 premature.
Of the 25 mature, 13 were putrid (52 per 100).
Of the 13 premature, 8 were putrid (61·54 per 100).

Of the 183 infants born alive:

| | | |
|---|---|---|
| The number that left the house was, . . | 125 = | 68·32 per 100. |
| ,, died, . . . . . | 40 = | 21·85 ,, |
| ,, remained Oct. 1, 1859, | 18 = | 9·83 ,, |
| | 183 = | 100·00 |

Left the house, 70 males, 55 females; = 56·0 to 44·0.
Died, . . . 19 males, 21 females; = 47·5 to 52·5.
Remaining, . 14 males, 4 females; = 77·77 to 22·23.

103 males, 80 females.

Of the 224 women delivered, 1 died; = 0·446 per 100.

The deaths of the 40 infants were due to the following causes, viz.:—

Convulsions, including some cases of congenital asthenia, 15; atrophy, 7; syphilis, 5; measles, 3; bronchitis, 2; tuberculosis, 2; pemphigus, 1; purpura, 1; cancrum oris, 1; hydrocephalus, 1; cholera, 1; cyanosis, 1. Total, 40.

Art. VIII. — *On Strangulated Femoral Hernia.* By E. Whittle, M. D., M.R.I.A., Senior Surgeon to the South Dispensary, Liverpool.

The interesting paper by Dr. Wilmot in the fifty-sixth Number of the Dublin Quarterly Journal has led me to look over some notes of several cases of strangulated femoral hernia, which have come under my notice during the last twelve years. In eight out of nine cases the operation was resorted to, and in every case with success: in one case the operation was not had recourse to, for, singularly enough, in the first instance the nature of the attack was not recognised by the surgeons in attendance, and the patient's life consequently was very nearly sacrificed by this error.

Case I.—A woman, about 46 years of age, of rather spare habit, had felt a small tumour in the left groin for the two previous years, from time to time. She had never had advice for it until this occasion, when it became painful and hard, and she found it impossible to return it. Her bowels were constipated for two or three days; there was a good deal of vomiting, but the matter was not stercoraceous. The taxis was carefully applied for some time ineffectually; the symptoms not being very urgent, it was determined to administer a full dose of opium, to apply cold, and to postpone the operation until morning. This was about eleven o'clock p. m.

At ten o'clock the next morning, the symptoms being more urgent, and the vomited matter in some measure stercoraceous, we decided, on consultation, to operate at once. The sac was quickly exposed, carefully opened, giving exit to about a drachm of fluid, when a knuckle of small intestine, about the size of a good marble, came into view; on dividing the stricture the intestine was easily reduced. The patient was relieved immediately, and recovered rapidly without a bad symptom.

Case II.—This case presented the same train of symptoms as the last, and the patient made a good recovery.

Case III.—In this case the patient was advanced in life, about 65 years of age, corpulent, and of a feeble constitution. She had suffered from hernia for many years, had worn a truss, but had worn it out, and had neglected to get a new one; when I saw her, the hernia had been strangulated for four days, and she was getting low and exhausted. The taxis had been already tried again and again ineffectually. I decided on proceeding at once with the operation; for this purpose chloroform was administered, and an attempt was made to reduce it by the taxis while she was under the influence of chloroform;

this attempt failed, and the operation was commenced. The quantity of adipose tissue that had to be cut through rendered it rather difficult to reach the seat of stricture; when the sac was opened, a mass of omentum, about as large as a pigeon's egg, was exposed, blackened, bruised, and full of coagulated blood, and entangled in the neck of the hernia was discovered a small knuckle of intestine, livid and congested, but apparently sound; as the stricture was divided, and the intestine slipped into the abdomen, a mass of fresh omentum was forced down by the patient coughing; this I gently returned. I then removed with the knife the most crushed parts of the old omentum, leaving in the wound what was adherent to the sac, and yet retaining some vitality. This patient at first went on very well, but on the fourth day the wound became erysipelatous and sloughy; however, no very serious symptoms followed; some of the omentum sloughed away, and she made a very good recovery.

Case IV.—A. H., a woman of thin, spare habit, 48 years of age, had felt a small tumour for a few weeks in the left groin, but it gave her no pain until a week before, since which time it has continued painful; her bowels have been obstinately confined; she has been very sick, and has now stercoraceous vomiting. The tumour in the groin is small, not larger than a marble, and very hard: no impression can be made on it by the taxis. A consultation was held, and it was decided to operate immediately; the patient having been now a week suffering from symptoms at least of incarceration, and for the last two days of strangulation. From the smallness and hardness of the tumour, I thought it prudent to operate with special caution, and it was fortunate that I did so, as the sac was found closely investing the black intestine, there not being a single drop of fluid in the sac. The intestine was easily reduced, and the patient was well in a few days, the wound almost healing by the first intention; there was no omentum in the sac. This case taught me an important lesson: viz., the danger of injuring the intestine by a too vigorous application of the taxis. I have seen the taxis applied with a force that I have no doubt would have been sufficient, in this case, to have ruptured the intestine.

Case V.—M. A. D., an old woman, 73 years of age; she had suffered from an old femoral hernia in the right groin for several years. I was called hastily to see her about three o'clock, p. m., on a winter's afternoon, under the following circumstances:—She had been seen by two of my colleagues the evening before; the symptoms were not then thought urgent,

and she was left in the care of our house surgeon until the next morning. My colleague—who was to have operated—was called out of town in the morning, and I had also gone out to visit, so that some time was lost before I could be found. When I first saw the patient, her state was so critical that I hardly felt inclined to subject her to the ordeal of an operation. She appeared approaching a moribund condition. Her pulse was very small and compressible, about 80, and occasionally intermittent; her countenance indicated a state of extreme depression; her voice was husky; her belly tympanitic, and there was continual vomiting of stercoraceous matter. The hernial tumour was about as large as a goose-egg.

There was evidently no time to be lost if any operation were to be performed; to have summoned my colleagues to a consultation would have caused a delay of two or three hours at least; so it was decided, after a little consideration between myself and the house surgeon, to give her the benefit of the operation. We gave her a little chloroform, just enough to lessen her sensibility, without inducing complete anæsthesia. On laying open the hernial sac, a great mass of disorganized omentum was sound occupying it, with a knuckle of intestine, about as large as a walnut, entangled in the folds of the omentum, in the neck of the sac, and very tightly strangulated. When the hernia was reduced, a mass of fresh omentum immediately slipped down; this was carefully replaced, and the whole of the disorganized omentum was removed by the knife without any hemorrhage. (This condition of the omentum was evidently caused by the application of the taxis.)

As nothing remarkable arose in the course of the operation, it was happily over in a very few minutes. She lost scarcely any blood, but felt very faint from the shock. We gave her brandy freely at first, and soon after a little opium. She rallied quickly, passed a quiet night, had her bowels freely open early in the morning; and in fine, made a quick and good recovery.

Case VI.—This was a case very similar to the first and second: the operation was performed in good time, and the patient made a good recovery.

Case VII.—J. J., a tradesman, 48 years of age, of florid complexion, but rather spare habit. I first saw him late on a Saturday evening: he complained of occasional pains in the abdomen; of a feeling of distention, as if from flatulence; of constipation; and of a feeling of sickness; but as yet there was no vomiting. He said he never had hernia, nor any enlargement in the groins; for fear of any oversight, I examined, and in

the left groin I found a little hard tumour, not a great deal bigger than a large pea; this tumour was exactly situated, if not in, on the situation of the femoral ring. I immediately suspected that I had a small femoral hernia in the male to deal with. When I questioned him about this lump, he insisted that it was nothing, and that it had always been there. He was a man of peculiar temper, in fact very eccentric, so that I did not place much reliance on his statement. On that night, and throughout the next day, I administered strong purgative medicines, and several enemata, without any good effect. He also had a warm bath, and the taxis was tried several times. He vomited frequently during the day, and was evidently getting gradually worse. I felt now convinced that there was a small hernia strangulated, and with some difficulty convinced him of the necessity of an operation. He was put fairly under the influence of chloroform, and I then carefully dissected down to the sac, which, when exposed, was not larger than the tip of the ring-finger. On carefully dividing it, the intestine came into view, closely enveloped in the sac, as in Case iv.; the sac containing no fluid; the knuckle was not larger than the tip of the little finger, very black, and very tightly strangulated. When the intestine was returned into the abdomen, the tip of the little finger could merely be inserted into the ring; the wound healed by the first intention, except a little bit between the two sutures, but the second day the patient was sitting up, and on the fourth he went out about his affairs; he never had a bad symptom. I think it very fortunate that the operation was performed so early in this case, as, from the condition in which the intestine was found, it must have very soon passed into a gangrenous condition.

Case VIII.—A tailor, fifty-six years of age. This was a similar case in the right groin, but larger; the sac contained some omentum, and a knuckle of intestine about as large as a wall-nut. He did not consent to the operation until the fourth day. At first he did well, but, two days after the operation, an attack of peritonitis set in, which kept him in a precarious state for some days, but ultimately he recovered very well.

Case IX.—In July, 1854, H. W. called on me, and gave me the following history of his case. He had resided in a tropical climate for about twenty years: he was now forty-four years of age. He was of spare habit, and had an inguinal hernia in the right groin. In the spring of 1852 he had a sudden attack of constipation, accompanied with vomiting, excessive pain, and great prostration. At the same time a very small tumour appeared in the left groin. The medical men in attendance ex-

amined this tumour with great care and anxiety, felt satisfied that it was not a hernia, but were not agreed as to its nature; he was subjected to very active treatment, bled freely, leeched, and blistered, without relief; at the end of eight days his case was considered hopeless. He was in this position, when a younger brother, arriving opportunely from England, insisted upon further advice being called in. Another physician was consulted, who recommended a prolonged warm bath, and repeated enemata of sweet oil; he was kept in the bath more than an hour, when he fainted. The bath and enemata had the desired effect; his bowels were freely moved, and he rapidly recovered. I should have mentioned that before the last physicion was called in, the small tumour had subsided. He had since several similar attacks of a milder character, always relieved by taking a smart purgative, the small swelling appearing with the attack, and disappearing upon the operation of the medicine. The medical men were still undecided about the nature of this swelling. Upon this case I felt it difficult to give an opinion; all the facts of the case seemed to point to a hernia as the cause, and he had a hernia in the other groin, but all the medical men who had seen the swelling were satisfied that it was not a hernia.

My friend was going to spend the autumn and winter in the neighbourhood of Leamington; thither he went, determined to come straight to Liverpool, if the symptoms should again return. In the following October an attack came on, apparently excited by a fit of coughing in church; not feeling relieved by a dose of aperient medicine, he came off by express to Liverpool. Upon examining the swelling, I found a small hard tumour, not larger than half a horse bean, and a little pear-shaped, occupying the situation of the femoral ring. I said at once, without any hesitation, "This is certainly a femoral hernia." His mind was so impressed with the idea that, whatever it was, it could not be a hernia, that I found it difficult to make any impression upon him; he was suffering a good deal from sickness, to relieve which I administered ice, then an opiate, and after an interval of an hour I tried the taxis ineffectually; I then tried a tobacco enema, with no better effect. The next morning we called in a physician, who agreed with me in my diagnosis, and we then tried the effect of chloroform and the taxis, when he was under the influence of chloroform. In this way the hernia was easily reduced, with complete relief of all the symptoms. He was provided with a suitable truss, and a few months afterwards he returned to South America, where he has ever since led a very active life, and never been troubled by

these attacks except, I think, twice, when on each occasion, he had allowed his truss to get out of order. Now, with regard to the dangerous attack in 1852, can there be any doubt that he then suffered from an attack of incarcerated femoral hernia? It could not have been quite strangulated, or he would not have recovered, considering the treatment adopted. How can we account for medical men in large practice making such a serious mistake? The comparative rarity of femoral hernia in the male, and the minute size of this one, probably caused them to ignore the probability of the swelling being caused by femoral hernia, their attention being, perhaps, too exclusively directed to the relations of the tumour to the inguinal canal. However, the mistake is pregnant with warning to others, never to lose sight of any point that may have the slightest bearing upon a case.

The history of these nine cases strongly corroborates Dr. Wilmot's opinion that in femoral hernia an operation should be early resorted to. In the case of H. W. it was not necessary; but it is very clear that in 1852 his life was very nearly sacrificed for want of the operation. In Case V. also, though the result was fortunate the operation was too long delayed. In the case of the tailor, No. VIII., his life was nearly lost by his putting off the operation to the last moment.

I think tobacco often objectionable on account of the very depressing effects that sometimes follow its use; and the chance of an operation being necessary, after all, makes the objection still stronger. Chloroform is, however, so much safer, and so much simpler in its application, and moreover, such a much more efficient aid to the taxis, that, for the last few years, I have abandoned the use of tobacco *in toto*.

My practice now is, first, a turpentine enema, a warm bath, and a full opiate; then the taxis; next the taxis under chloroform: all these failing, to operate; but with the aid of chloroform I find hernia generally so reducible, that for the last two years I have not had occasion to operate for this affection.

In conclusion, I may say that I think Sir Astley Cooper's hernia knife an awkward and objectionable instrument. The long blunt point always felt to me as if it pressed too tightly against the intestine, and I could not help fearing that if the intestines were distended with gas, the point might perforate it. On this account, I early in my career rejected it, and I have ever since operated with a plain probe-pointed bistoury with a slight curve. I have perfect command of this instrument, and operate with it with the greatest facility.

Art. IX.—*Contributions to the Surgical History of Syphilis.* II. *On Syphilitic Stricture of the Œsophagus.* By James F. West, Surgeon to the Queen's Hospital, Birmingham.

The cases which I propose to cite on this subject will, I think, bear me out in giving the above written title to this paper. They are, as far as I know, or am able to learn, unique, and although some of my professional brethren, following the dictates of experience and the highest surgical authorities, may be inclined to doubt the existence of such a disease, still I shall be able to prove, without fear of contradiction, that such an occurrence may and does occasionally arise in the history of aggravated cases of constitutional syphilis.

The actual existence of syphilitic stricture of the œsophagus has often been surmised; numerous cases bearing upon this point have been at different times reported; but all, having wanted the final proof of a pathological examination, have been considered unreliable, and of insufficient importance to warrant their being brought forward as legitimate instances of the disease.

I intend going fully into the consideration of this subject, and shall give the particulars of several cases which are to be found among the older writers on syphilis, in which, had the history been fully investigated and brought to a close in the post-mortem room, ample opportunity would, I believe, have been afforded for setting this *quæstio vexata* entirely at rest.

I will first give the report of the case on which I mainly found my observations; then examine the various authors who have bestowed attention on the subject; and lastly, bring forward a case which is now under my treatment, in which there is but too great reason to fear that this fearful disease has invaded the œsophagus.

Jane M., aged 21, single, of 14, Nelson-street, Birmingham, was admitted into the Queen's Hospital, May 18, 1858. The dresser's account, taken at the time of admission, states that she is a delicate girl, and has many times been an applicant for hospital relief, with various forms of secondary syphilis. At one time her hearing was much impaired, and, after a purulent discharge had continued from the left ear for some months, a necrosed fragment of the temporal bone was discharged. At another time she had a scaly, cutaneous eruption, and during the last six months she has suffered more or less severely with sore throat. She has had two children; her last

confinement being in October, since which time she has not menstruated.

She had gonorrhœa three years ago, but denies having had either sores or a bubo. The discharge lasted at that time for about a month; it again showed itself previous to her delivery in October; but since that time she has not noticed it at all. She has had what she calls "rheumatic pains" in the arms for a long time, but never had any distinct attack of acute rheumatism. She now complains that she cannot swallow anything, and that her food returns during the act of swallowing: about twice a day she is able to swallow without much difficulty. She has no pain except during deglutition. The throat is very much ulcerated, both tonsils, as well as the fauces and soft palate, being involved: the surface is ash-coloured, with red patches dispersed here and there. She is much emaciated, and very anemic. Pulse weak; tongue clean; bowels open; urine scanty, but clear and healthy. Ordered to take five grains of iodide of potassium in an ounce of decoction of bark three times a day, and to use the hydrochloric acid gargle. Diet—milk, arrow-root, beef-tea, and three ounces of wine daily.

21st. She was about the same, both as to the condition of the throat, and as to the power of deglutition: she was ordered to have the fauces sponged night and morning with a solution of nitrate of silver, one grain to the ounce, and to use mel boracis as a gargle.

This treatment was persevered in till the 28th instant, but without benefit; she got weaker, and the ulceration of the fauces became more extensive, while swallowing was even more difficult; she took scarcely any food, and had no relish for anything. An attempt was made to pass a small-sized œsophagus bougie, but it was ineffectual. The throat was ordered to be touched each day with linimentum æruginis; and she was to take quinia in three-grain doses instead of the iodide of potassium, with an occasional dose of morphia at bedtime. For some days the linimentum æruginis appeared to give relief, and she could swallow fluids a little better; however, her strength diminished, and she was unable to take the quina in consequence of its affecting her head.

Attempts were made every other day to pass a gum-elastic catheter, and, although the stricture could not be penetrated, yet, when pressure was made against it for some minutes, she generally found that she was enabled to swallow better after a few hours. Citrate of quinia and iron, in doses of five grains three times a day, was substituted for the quinia on the 19th of June, and a gargle, consisting of equal parts of liquor hydrar-

gyri bichloridi and water, in the place of the linimentum æruginis.

No improvement occurred from this change in the treatment. She was gradually getting lower, and the progress of the disease in the throat was not arrested. I, therefore, determined, on the 10th of July, in spite of her debilitated state, and of her having, as an out patient, been more than once treated with mercury either in the form of blue or Plummer's pill, to give that remedy again a trial. She was ordered to take ten drops of the liquor hydrargyri bichloridi in decoction of bark three times a day, and to have mercurial ointment rubbed into the axillæ. By the 28th of July the gums had become spongy; and, consequently, all mercurials were stopped. For some days slight amendment was perceived; deglutition was less painful and difficult. She asked for ale and cider, and fancied she could swallow them better than milk or other fluids. She was also allowed brandy and eggs.

August 6th. She had relapsed into her former condition, and could not swallow anything for hours together; all attempts at deglutition being attended with the most painful straining and spasm. Small blisters were now applied on each side of the neck, in the hope that counter-irritation might be serviceable; however, the expectation proved fallacious. At this time I left town for a few days, and she was placed under the care of my colleague, Mr. Langston Parker. He resolved upon resorting to mercurial fumigation, and for that purpose ordered ten grains of calomel to be inhaled every night and morning. The report in his case-book, taken at that time, states "that she had now at times complete dysphagia for hours together, at one time for as many as thirty-six she could not swallow anything. Neither an ordinary œsophagus bougie, a small catheter, nor an ordinary urethral bougie, could be passed, but when an instrument was carried down to the apparent point of obstruction, and pressed gently against it for a short period, the patient could swallow liquids a few hours after. When this was done in the morning, she generally swallowed in the afternoon. She could not swallow brandy and milk, or beef-tea, but she could swallow cider, and at all times."

The bougie was used daily, and it gave some temporary relief. The mercury affected the mouth sensibly, and was then discontinued. Towards the end of August it was evident that her case was desperate, and that the constriction of the œsophagus could in no way be relieved. A consultation of the medical staff was held, and the question of gastrotomy was raised, but the prospect of success was deemed so slight, owing to her

very enfeebled state, and to the existence of vomicæ having been discovered in the apices of each lung, that the idea was abandoned. She was recommended chloric ether and morphia to ease her sufferings, and a liniment consisting of half an ounce of extract of belladonna to three ounces of linimentum saponis, to use to the throat.

She died exhausted on the 2nd of September. The necropsy was performed by Dr. Bond, and from his pathological register I made the following extract:—

*Body* much emaciated. *Lungs:* both more or less adherent to parietes of chest; the right being also inseparably attached to the diaphragm. Both lungs also contained tubercular deposit in different degrees of softening, with several small cavities in the upper lobe of each, one in the left apex being as large as a pigeon's egg. *Heart:* tissue soft; valves healthy, but much reddened, as was also the lining of the aorta. *Stomach* contained about a quart of green viscid fluid. The upper portion of the *œsophagus* for about four inches was much dilated; its mucous membrane thickened, and marked by spots having the appearance of recent cicatrices. At this distance from the upper end it was suddenly constricted, and terminated in a narrow canal which would barely admit a No. 4 catheter. This constricted portion, which was about two inches and a half in length, was formed by the thickening of the mucous membrane, and by fibrous deposit in the form of bands and bridles, having very much the appearance of an old stricture of the urethra. Below this track the œsophagus continued perfectly healthy to its termination in the stomach. *Liver* weighed 2 lbs. 4 oz.; was soft, and apparently fatty: the surface marked by fibrous streaks, and its capsule in part slightly thickened. *Spleen* softer than normal, with its capsule closely resembling that of the liver in appearance. *Kidneys* pale and soft, but apparently healthy; capsules slightly adherent. *Uterus* and *vagina* presenting nothing unusual, with the exception of a small suspicious-looking spot on the lower part of the orifice of the latter (cicatrix?).

The fact of stricture of the œsophagus in this case is perfectly patent, and it remains to consider whether any cause, other than that to which I have referred it, can have produced this lesion. Certainly, no positive proof of the primary sore can be advanced; but the secondary symptoms extending over a long series of years are distinctly marked; and in how very many cases do we find persons in the class of life of this girl denying that

they have ever had sores, when their effects are but too plainly visible in the scarred skin, the ulcerated throat, and the carious ossa frontis and tibiæ of the unfortunate sufferers? Such a sore, no doubt, existed at some period of her career, and its probable site was manifested in the post-mortem examination by a cicatrix at the lower part of the vagina. We have no account of the swallowing of any caustic or irritating fluid, so that we cannot attribute the stricture to that cause. But we find a patient gradually exhibiting the symptoms of well-marked syphilitic cachexia, and also experiencing frequent ulceration of the fauces and pharynx. At first, slight dysphagia alone led to the suspicion that there was narrowing of the œsophageal canal; but, with its persistence and tendency to become worse, the suspicion became augmented, until at last all doubt was removed on an attempt being made to pass a small œsophagus bougie.

Aggravation of the local symptoms, and deterioration of the general health, kept pace with each other. From being able in the early stage to swallow small lumps of solid food, she was obliged, in course of time, to confine herself to viscid and glutinous nutrients, as beef-tea and soups; and, towards the last, thin acid fluids, such as cider, could alone be taken. The post-mortem examination revealed just the condition of parts that might have been anticipated: contraction of the lower part of the œsophagus for a considerable extent, and dilatation of its upper part from the continued pressure of fluid and solid matters upon its walls. To the inanition which arose from this contraction of the œsophagus, death is obviously referable, for, though the lungs were found to be diseased, there was not sufficient destruction of lung tissue to have led to such an issue, had the case been uncomplicated with other organic mischief.

The presence of numerous recent cicatrices clearly indicated that ulceration had existed in the walls of the œsophagus. The deposit in the submucous tissue was fibrous; it was exactly similar in nature to that which is so well described by Dr. Wilks[a] as characteristic of syphilitic ulceration, and could not under any supposition be referred either to cancerous or tubercular degeneration.

[a] Pathological Anatomy, page 258.—"When a syphilitic ulcer is examined after death, it may generally, I think, be told, for the reason I named when speaking of the larynx, by the adventitious fibrous deposit or lymph, which is formed in the tissue; and thus, besides the ulceration, you find the adjacent parts much thickened and indurated."

It is remarkable that pathologists have made little or no mention of this form of disease of the œsophagus.

Rokitansky, usually so ample and lucid, in his descriptions of the morbid phenomena of the various viscera, does not refer to it, though he carefully classifies the "textural diseases" of the œsophagus thus—

"1. *Inflammation.*—*a.* Catarrhal. *b.* Croupy (exudative). *c. Pustular.* *d.* That which is produced by caustic substances.

"2. *Softening.*

"3. *Morbid growths.*—*a.* Fibrous and fibro-cartilaginous tumours and polypi. *b.* Tubercular deposits. *c.* Carcinomatous affections, including both scirrhus and medullary sarcoma;" and in another place he alludes to the subject of contraction of the œsophagus in the following manner:—"The fauces and œsophagus are not unfrequently subject to contraction, from being compressed by the enlarged thymus gland, by aortic aneurisms, adventitious growths, &c.; but the contractions resulting from textural changes in the coats are of much importance; and among these we must more particularly allude to stenoses brought on by cicatrization after corrosion, by caustic substances, and by cancerous affections (cancerous strictures)."

No distinct allusion is made to this cause of contraction of the œsophagus by Dr. Handfield Jones[a], and although such an inference might be drawn from this paragraph,—"the œsophagus is liable to be *constricted*, either by the compression of external growths, or by cicatrices in its own walls, the result of former ulceration or sloughing; or by cancerous formations in its coats:"—still it is more than probable that the author's attention is directed to the ulceration or sloughing which follows the imbibition of caustic fluids, rather than to that which has its origin in the syphilitic virus.

Dr. Wilks[b] admits the existence of such a kind of stricture

[a] Manual of Pathological Anatomy. By Dr. H. Jones and Dr. Sieveking.

[b] "*Dilatation.*—Cases are recorded where the œsophagus has been dilated. I have never met with a case of the kind, not even where stricture exists, for the reason, I suppose, that little or no food is taken in such cases, and the disease, which is generally cancerous, runs too rapid a course for dilatation to occur.

"*Contraction* or *stricture* is a form of disease very commonly spoken of, as if the œsophagus could be strictured in the same way as the urethra. This, however, is exceedingly rare, for in the vast majority of cases where obstruction exists, it is due to disease within the tube, or to some tumour pressing from without: thus, if the cause be an organic one, a cancer of the œsophagus or aneurism of the aorta may generally be predicted. As, however, it is not often certain in the living subject that such severe affections cannot exist, the term 'stricture' must still be conventionally used in those cases where there is a mere difficulty of swallowing; remembering that it is exceedingly rare to demonstrate such a condition, for during several years' obser-

and describes from analogy, what the character and appearance of such a stricture would be: he has not, however, seen any case in the post-mortem room either of contraction or dilatation of the œsophagus; and, although he refers to a specimen of the latter disease in the Museum of Guy's Hospital, which he thinks may be referable to syphilis, he quite ignores the idea of the œsophagus being strictured "in the same way as the urethra." Nevertheless, in the case which I have related, I quote Dr. Bond's own words, when I say that this specimen had "very much the appearance of an old stricture."

Lastly, Nélaton[a], after describing most thoroughly the different kinds of contraction of the œsophagus, hints at the possibility of it arising from secondary syphilis, but evidently is indisposed to admit the probability of such an event. This is indicated in the following sentence:—"Pour compléter cette longue ènumeration citerons nous les rétrécissemens syphilitiques? Ceux-ci ne paraissent pas établés sur des observations assez exactes, pour que nous pouvons les admettre sans aucune reserve, et si certains cases de rétrécissemens ont pu être guéris par les préparations mercurielles, cela ne prouve pas rigorousement qu'il s'agissait d'une affection syphilitique."

Recent authors on this subject are equally silent.

Mr. Langston Parker, in his valuable work on Syphilitic Diseases, appears from the following quotation to consider such an event as stricture of the œsophagus unlikely, as one of the sequelæ of secondary syphilis:—"Persons who have suffered from constitutional syphilis frequently complain of pains in the throat, increased by deglutition, and referred to various points about the pharynx and larynx: the parts which are the seats of those pains have previously been affected by ulcers or inflammation; but, although the ulcers had healed, the pains remained. I have frequently been unable to detect any lesion in parts thus affected, and been led to regard many of these cases as pure syphilitic "neuroses."

Mr. Parker, however, informs me that since his last edition

vation in the post-mortem room, I have never yet met with an instance of it. We have here, however, from the Museum one or two specimens, which seem to show that an ulcerative process may give rise to a cicatrization and subsequent contraction.

"Here is such a contraction [referring to a preparation in the Guy's Hospital Museum] at the lower part of the pharynx, where it joins the œsophagus; it is, no doubt, a contracted ulcer, and may be syphilitic. Here is one with an ulcer of a doubtful character; and this œsophagus from a boy is a very remarkable specimen, showing great thickening of the wall at its lower part; this had caused much difficulty of swallowing, and was probably due to the imbibition of some irritating fluid during infancy. The other cases I shall presently show you in connexion with cancer."—*Wilks*, page 262.

[a] Élémens de Pathologie Chirurgicale, vol. iii., P. 410.

appeared, he has met with one case in private practice in which unmistakable stricture of the œsophagus existed. It occurred in the person of a gentleman who had led a very gay life, and who had suffered severely from primary and secondary syphilis. In him the dysphagia was so extreme that great weakness and emaciation rapidly ensued. Thinking that a sea voyage might do him service, he started for the Cape of Good Hope, but died exhausted on the passage.

Mr. Acton[a] details the numerous varieties of syphilitic sore throat, giving a full account of their history and progress, and yet does not advert to the occurrence of stricture as one of the terminations of extensive and long-standing ulceration of the pharynx.

In his instructive chapter on the causes of death from syphilis, no mention is made of such a cause, nor is there any allusion to it in his account of the symptoms of that state of the system, embracing many of the complications and sequelæ of tertiary syphilis, to which M. Ricord has given the name of "syphilitic cachexia." No case illustrating this feature of syphilis appears in the works of Vidal de Cassis, Ricord, or Swediaur; and, in fact, all modern surgeons seem to have received implicitly, and coincided with the opinion which John Hunter[b] enunciated, namely, that "there are also a number of diseases described by authors as venereal, especially by Astruc and his followers, which are almost endless," making special mention among them to that "*wasting* which results from want of nourishment."

In the older writers on syphilis we find frequent allusion to the fact of dysphagia coming on in the course of confirmed syphilis, and producing by its continuance great emaciation and debility; and in the works of Carmichael and Turner cases are related, which give great weight to the opinion that stricture of the œsophagus has been from the time of the earliest ravages of syphilis one of the protean phenomena which that disease has exhibited, when the virus has fully developed itself in the system, and become constitutional.

Astruc[c], in his fourth book "On the Œtiology, Diagnostic, Prognostic, and Cure of the Venereal Disease when it has become universal," speaks of "the mucus of the fauces, if it partakes of the venereal infection," producing "inflammation of the tonsils, uvula, and fauces, with a difficulty in swallowing;" and he goes on to state that "by degrees it becomes acrimo-

[a] Diseases of the Urinary and Generative Organs.
[b] Palmer's edition of Hunter's Works, vol. ii., p. 423.
[c] Translation by William Barrowby, M. B., published in 1737.

nious, and then, by corroding these parts, it forms ulcers; from hence proceed ill-digested, malign, stubborn ulcers of the tonsils, uvula, fauces," &c.—thus clearly distinguishing the difference between the slight affection of the throat which attends the early stage of secondary syphilis, from that deep, far-spreading ulceration which destroys the walls of the pharynx and œsophagus, and ends in their contraction under the influence of cicatrization.

Falck, in his treatise on Venereal Disease, published in 1774, after comparing the symptoms of Job's illness to those observed in aggravated cases of lues, asserts that, among the final symptoms denoting that "a crisis is now at hand," are "difficulty of speech, swallowing, and respiration;" but does not give any cases in illustration[a].

A very interesting case, which evidently shows that the disease had attracted the attention of some of the earlier writers on syphilis, is given by Dr. Daniel Turner[b]. He gives the history of a "person several years advanced beyond his meridian, and, for many of these last past, a great friend of the bottle," who became exceedingly emaciated, and suffered much from a cough, which his physicians attributed to phthisis. On his being placed under Dr. Turner's care, he, according to Dr. Turner's own graphic description, "appeared a very frightful object." He says: "On depressing the tongue, I found the uvula gone, with the greater part of both amygdalæ, the whole palate, and behind, on the muscles of the pharynx, as far downwards as could be seen, over-run with *depascent ulcers* covered with sordes; and it was with much difficulty, holding my ear to his mouth, that I could understand one word in three, though straining himself with his utmost effort to pronounce them. I then applied to his relations, inquiring how he got down his nourishment, who told me that his liquor, here of late especially, though given by spoonfuls, returned instantly by the passage of the nostrils; and that any other aliment, as chicken-broth, with bread or panada, could find no admittance to his stomach; for that the muscles inservient to the swallow, being preyed upon by the virulent humours, tied down, as it were, and withal so tender, that they were unable to perform their office; insomuch, that in *like manner with those labouring under a cancer of these parts, he was in real danger of being famished, through want of conveyance of sustenance to the same.* This was, indeed, the real case of this unhappy man, who, as I under-

[a] Page 277.

[b] A Practical Dissertation on the Venereal Disease. By Daniel Turner. 1732. Part I., p. 306.

stood by his friends, had not for many days past been able to let down his nourishment of any kind."

A previous attack of the venereal disease having been admitted by the patient, he was at once put under cinnabar fumigations, and with so much advantage, "that the miserable man, who just before could not let down one drop of broth or gruel of any sort, by the time a month was expired, devoured a large chicken at a meal; his drink also, taking heed in swallowing leisurely, passing with very little rising up to the passage through the nose."

"The fume did all in this surprising recovery."

The nature of the disease in this case cannot be questioned: that narrowing of the œsophageal canal existed is amply demonstrated by the symptoms, and that such was the doctor's own impression is evident from his comparing the symptoms to those which would be found in any well-marked case of cancerous degeneration. The success of the mercurial treatment further assists in proving the correctness of Dr. Turner's diagnosis.

Carmichael[a], among his many varied and ably reported cases, narrates two which bear upon the subject, and which, though wanting in positive evidence as to the fact of stricture of the œsophagus having been clearly shown, still are useful as assisting in its elucidation.

Luke Boland was admitted "on account of an extensive ulcer of the back of the pharynx, the entire of which, as far as could be seen, was engaged in the ulceration, and covered with a white, slimy adhesive matter. The velum and surrounding parts were inflamed; and he swallowed with pain and difficulty."

After three weeks' use of the antimonial solution, with a gargle of borax and honey, he was ordered "mercurial frictions." For two months this treatment was persevered in, although at the end of three weeks "his mouth was severely affected by the mercury." Upwards of six ounces of ointment were used, and that very little benefit resulted from it is shown from the report, which states that "the ulcer of the pharynx looked foul," and "was almost as extensive as at the time of admission." Sarsaparilla in powder and decoction was ordered. This he took for four months as an out-patient, when "he was again admitted with the same ulceration as before on the pharynx."

Recourse was again had to mercurial frictions; and for six

[a] Essay on the Venereal Disease. 1814.

weeks salivation was kept up. Amendment was followed by relapse; but at the end of thirteen months he was after a third course of mercury discharged cured, with directions to return if he again became ill.

"Pain and difficulty of swallowing" in such an aggravated case of syphilitic disease of the pharynx may, with great show of probability, be referred to the contraction of cicatrized ulcers, rather than to any other cause.

The following case, that of Margaret Malone, presents a very similar history; but the symptoms of stricture of the pharynx and œsophagus are more marked:—

"Difficulty of swallowing" was throughout the prominent symptom; and after a lapse of six months, during which time she had been mercurialized several times without the slightest benefit, she was removed from the hospital, the ulceration in the throat "continuing to extend;" and her friends seeing no hope of her recovery. She died shortly afterwards; but in Carmichael's report her death, strangely enough, is attributed rather to the "exhaustion of her strength" than to the ravages of the ulcer.

Is it not more likely that the true cause of the exhaustion was a narrowing of the canal of the œsophagus, which prevented her taking sufficient nourishment to enable her to bear up against the severe and protracted ulceration which arose from the attack of syphilis, and from the prostration of the vital powers which followed the oft-repeated administration of mercury?

Having examined in detail, and with some diffidence, the literature of this department of syphilis, I now proceed to the narration of the second case of stricture of the œsophagus which has fallen under my notice, and this I shall do briefly, for this paper has already exceeded the limits which I had proposed for it; and as I fear that there is a probability of my being able before long to present a pathological account of the case in question, it is the less necessary for me to dwell at length upon the morbid condition in which the œsophagus now is, and upon the symptoms to which that condition gives rise.

H. P., an "unfortunate," twenty-five years of age, tall, and formerly of comely aspect, dark complexioned, and of nervous temperament, the child of healthy parents, was seduced at the age of 15. She left home soon after, and before twelve months had elapsed she became the subject of primary syphilis; a bubo formed, and suppurated. A discharge from the vagina

continued for twelve months, during which time she remained without proper medical treatment. At the end of that time she returned home in a very weak and prostrate condition: mercury was administered without producing extreme salivation; and her health was improved by generous living and rest.

Secondary symptoms soon, however, began to manifest themselves, and since that time she has never been free from them. Almost every variety of syphilitic cutaneous eruption, rupia, lepra, and acne, each and all have in turn affected her; she has had nodes on many of the bones, ending in caries of the skull at one time, and of the os unguis at another, and in the latter instance giving rise to fistula lachrymalis. She has repeatedly been the subject of sore throat; the uvula and amygdalæ have been destroyed, and the soft and hard palate have been honeycombed by ulcers at various times, so that there are now several fistulous openings communicating with the nares: latterly, aphonia has been present for weeks together, from implication of the larynx; and dysphagia has existed with greater or less intensity for the last twelve months. During that time her strength has gradually been diminishing; and she is now a mere shadow of her former self. She cannot, at the present time, swallow any solid food, and often for several days even fluids refuse to pass, and regurgitate by the nose and mouth. The throat has an ashen-gray appearance, is covered with thin frothy mucus, and has the marks of many old as well as recent cicatrices: the whole of the posterior wall of the pharynx is gone, and only a thin, but dense, cicatricial membrane appears to cover the vertebral column. About four inches down the œsophagus an obstruction is met with, when a medium-sized œsophagus bougie is passed, but a No. 12 urethral catheter can be introduced with but a slight hitch.

Every kind of treatment has been adopted, and frequently great benefit of a temporary nature has been experienced. Iodide of potassium gives some relief. She has taken an immense quantity of it, and has found it more useful than anything else: she now takes it in five-grain doses, three times a day; and uses a gargle of hydrochloric acid. She asserts that she has never had but one attack of syphilis, and that she has not even had a discharge for eight years.

Such is the history of this poor girl. It is brief, but suggestive, and leaves, I fear, no reason for hope that the disease may be eradicated, or that her life may be prolonged many months beyond the present time.

Many will probably hesitate before believing that such

cases are really to be met with. I can only say, that they caused me no little surprise, and that when, on inquiry among my medical friends, and upon research among our highest authorities, I found that no cases of an exactly parallel description could be met with, I conceived it incumbent on me to lay before the profession as full an account of them as possible, together with such remarks as appeared to me to arise out of the consideration of them.

(*To be continued.*)

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Art. X.—*On the Operation for Cleft Palate.* By S. Browne, R.N., Licentiate of the King and Queen's College of Physicians in Ireland, and Surgeon to the Belfast General Hospital.

In the twenty-fifth Number of this Journal, February, 1852, I published a case of cleft palate, in which I had operated with complete success. In that paper I pointed out one or two peculiarities in the operation I had adopted, and which I regarded as improvements. Since that time, I have had experience of five additional cases, and I still believe that the method formerly referred to, by which the sutures are inserted and made fast, is unquestionably an improvement upon that adopted by others. With regard to the lateral incisions which I practised in my first case, and which I then considered improvements, I have modified my views, as the steps of the operation I now perform are different, in many respects, from those I formerly pursued. From the period that Roux brought the operation for cleft palate into notice, up to the present time, several able papers have been written on the subject, and many eminent surgeons have modified and improved the method originally recommended: hence, I do not flatter myself that I can shed any new light upon it, or add anything of practical value to what is already well known; but as I hold that it is the duty of every practitioner to record his experience of rare or interesting questions in medicine and surgery, I venture to place before the profession the views which I now entertain regarding staphyloraphy.

To bring these clearly out, it will be necessary for me to go back to my early operations, and compare the plan I then adopted with that which I have recently put in practice. Although my first operation for cleft palate was perfectly successful, a review of the steps of that operation, and of the case as compared with the condition of the next patient that pre-

sented, led me to see that the comparatively simple operation I then performed could not possibly succeed in the second case that offered, and hence I learned that probably each case of cleft palate required to be carefully examined, so as to ascertain what modifications in the operative proceedings might be required. For instance, in one patient we shall observe that the fissured velum, when at rest, hangs down, the two margins of the cleft closely approximating; and that, even when the muscles are thrown into action, the margins are not drawn much apart; while in the next, perhaps, there may be a wide gap at all times in the velum; and when muscular action is excited, the separate portions are drawn outwards and backwards very far apart indeed. In another, we shall not only observe that the velum is fissured, but that the hard palate is cleft also. In each of these cases it is obvious that different steps will be required to afford an operation a fair chance of succeeding, and hence it will be necessary to adopt certain proceedings in the one case that would not be required in the other.

Thus, in my first case, the cleft velum had the margins of the fissure, when at rest, nearly in apposition, and I was enabled, after I had pared these margins, to bring them together with sutures, without any difficulty; while I practised extensive lateral incisions parallel and close to the margin of the bony palate, to overcome motion and to compensate for non-division of any muscular structure before introducing the sutures.

In my next case, I found it requisite to divide the levator and tensor palati muscles of each side, with Mr. Fergusson's knife, before I could succeed in paring the margins of the fissured velum properly; and even after the parts were brought into apposition, I had to make lateral incisions, as in my former case, to prevent some undivided fibres of the levatores palati muscles from drawing the velum upwards and backwards. This case did not succeed, probably from my having neglected to divide the palato-pharyngei muscles. I again operated on this patient after the lapse of nine months, but the operation failed once more.

My third case required a double operation, as both the velum and bony palate were cleft. I first operated with the view of closing the fissure in the velum, and divided the palato-pharyngei and levatores palati muscles, and after the sutures had been inserted and fastened, I divided the fibres of the tensor palati on each side, by making incisions through the entire thickness of the soft parts; these incisions commenced on either side, exactly opposite the hamular process of the sphenoid, and extended forward along the border of the hard palate for

about half an inch. I likewise found it necessary to free the soft parts from their attachment to the posterior portion of the cleft in the bony palate, so as to allow the apex of the fissure in the velum to be brought perfectly together. This operation required the insertion of five sutures, and turned out very well. After some months, I performed Mr. Warren's operation on the hard palate; the steps of this operation were, first, paring the entire margins of the cleft, including the part where the fissure in the velum had been united, and then, with a suitable knife, detaching the soft parts from the bone, all around the cleft, to the extent of fully half an inch; this I found to be a very difficult and painful part of the proceeding; having, however, succeeded in freeing the soft parts, to the extent I have named, I was enabled to bring their edges together with two points of suture; these sutures I allowed to remain in for five days, at the end of which time the parts seemed united; but on the next day I observed that ulceration had occurred at the anterior portion of the cleft, and that a small button-hole remained. This, after some weeks, contracted considerably, yet still an opening, a quarter of an inch in length by two lines in breadth, was left. I wished to attempt to close the aperture by a fresh operation, but the patient would not consent, especially as his powers of deglutition and speech were much improved, and he was quite content "to let well alone," and perhaps he was right, as, in another case, when I made the attempt to close a somewhat similar button-hole, remaining after staphyloraphy, I failed, and matters were anything but improved by my interference.

In my fourth case I followed exactly the instructions given by Mr. Fergusson in the last edition of his excellent "Treatise on Surgery," and divided the palato-pharyngei and levatores palati muscles, as he directs. I then inserted three points of suture, brought the parts into apposition, but did not divide any of the fibres of the tensor palati from the front. The parts opposite the anterior and posterior points of suture united firmly, but at the central point the union gave way after the removal of that suture on the fourth day, and that occurrence I attributed to the action of the undivided fibres of the tensor palati of either side. After some time I pared the edges of this aperture, divided the fibres of the tensores palati, and brought the parts together by means of my double suture, and the case then succeeded very well.

In my last two operations on the palate, I took especial care that every muscular fibre that was at all likely to cause any motion in the soft velum after its separated portions had been

brought together, should be divided; and this I deem essential to the success of staphyloraphy; for, while muscular action of the part can be continued, even in a very moderate degree, there must be reason to apprehend that union at every point of the joined fissure will not take place. Now, if we attentively consider the anatomy of the soft palate, and the action of the several muscles that principally constitute its structure, we shall see that in the act of deglutition and speaking the velum has a good deal of motion; and if we look into the fauces of a patient, the velum being in a normal condition, we shall observe that this little curtain is drawn somewhat upwards and backwards, and made tense, seemingly by an involuntary action of its muscles. Then, in the cleft condition, each half of the velum will be observed to have this involuntary muscular action greatly exaggerated, the separate parts being drawn backwards and a little upwards, while its edges are approximated, but principally in the divided uvula, this latter being caused by the action of the azygos uvulæ. The muscles which cause this motion seem to me to act in the following order of importance so far as the success of the operation for the cure of cleft palate is concerned, namely—the levator palati, the palato-pharyngeus, the tensor palati, the azygos uvulæ, and the palato-glossus of each side. The action of the azygos, while it interferes somewhat with the steps of the operation, exerts very little, if any, influence over the healing process; while the palato-glossii, I imagine, scarcely interfere with the cure. The muscles, then, to which we are principally to direct our attention, and which require to be divided, are the levator palati, the palato-pharyngeus, and the tensor palati on the right and left sides. The next question that arises is, in what manner can these several muscles be most satisfactorily divided? From my own experience, I believe that the levator palati, and the palato-pharyngeus can be most effectually divided by the method adopted by Mr. Fergusson; while the tensor palati, and, probably, some fibres of the levator, can be best divided by the incision from the front, as practised by Mr. Pollock. One thing, however, is quite certain, *that motion of the part must be overcome,* no matter what incisions the operator may deem it necessary to make! Next after the division of the muscles, and certainly of equal importance, comes the paring of the margin of the fissure; now, if this be not completely done, it is clear that union throughout cannot take place; hence the operator must see that no portion of the edge of the cleft has escaped his knife; from the point of the uvula, on each side, to and around the apex of the fissure, the entire margin must be

fully removed, and this can only be effected by a very careful use of the knife, so that a tolerably deep surface shall be left on both divisions of the velum, to be brought afterwards into apposition; consequently, a considerable breadth, fully one-sixth of an inch, should be removed.

The next matter of importance in the operation is, the suture which should be used, and this question embraces the material of which the sutures shall be made, whether they shall be single or double, and the best method of inserting and making them fast. On these several points my experience leads me to offer a decided opinion. In the first place, then, I consider that good white sewing-silk, that known as staymakers' silk, is the best material; this is made very strong and perfectly even, and, when waxed, can be knotted and fastened with the greatest facility. With respect to the next point, whether each suture should be used single or double, I am satisfied that the double suture is the better, and this I have used in all my operations on the palate. In my first paper on staphyloraphy, I drew attention to the method I adopted in using the double suture, and I then said of it, what lengthened experience has confirmed, in my mind, as true, namely, that after the double thread had been passed through both sides of the soft palate, the cut surface could be brought easily into apposition by passing a single end of the suture, on one side, through its duplicature, or "bight," on the other, and then, by pulling gently, the loop of the ligature would slide freely along its single portion, until the parts came closely together. It has been recommended to use a single thread for a suture, and to fasten it by running one end through a simple knot on the other, and thus, by drawing upon the running end, bring the parts into contact; but there is this objection, that, should the thread be uneven, it is likely the knot would jam too soon, and thus the suture would become useless; moreover, it appears to me, that the single ligature is very likely to tighten so much, in fact, before the proper time, as to require an injurious force to bring the cut surfaces of the velum together; while, on the contrary, the double thread, applied as I advise, must effect apposition of the pared margins of the palate without any injurious force having been required, and without any risk of tearing or cutting through its substance.

In inserting the sutures, great care should be taken that the entire thickness of the velum shall be included, both mucous surfaces being transfixed, as nearly as possible at the same distance from the edge of the wound; to effect this, it will be necessary to pass the needle from the front on both sides, so as to

insure the insertion of the sutures in the way I recommend, and, when they have been so passed, the ends can be very easily secured by a common knot, as beads, perforated shot, or any appliances of the kind, are quite uncalled for. The distance at which the needle ought to be entered in the velum should be *fully* one-sixth of an inch from the pared margin, and in some cases it might be well to include a quarter of an inch. While speaking of ligatures and the method of fastening them, I may mention that Dieffenbach used metallic sutures, and united the two parts by twisting them together. His mode of proceeding was evidently very clumsy, and the twisted wire knots must have surely caused great irritation by fretting the upper surface of the tongue. In thinking over the possibility of using metallic sutures in staphyloraphy, the idea has been suggested that the lead splint, with silver wires, as used by Dr. Robert Battey of Rome, Georgia, United States of America, for the closure of vesico-vaginal fistula, could be applied for the cure of cleft palate. When that gentleman visited Belfast, some months since, he did me the honour of explaining his method of applying his splint and sutures, and I have since thought that his little apparatus might answer very well in the operation of velo-synthesis. I have already tried his splint and wire sutures in hare-lip with the most satisfactory results.

As it would be almost impossible to convey, in words alone, any clear idea of this apparatus, or the mode of using it, to persons who had not seen it, I shall not refer further to it now, but merely say that it seems well suited to fulfil the purpose which its accomplished inventor intended.

Having thus stated my views on the several important points of this interesting subject, I shall now proceed to mention, in detail, the steps I follow in operating for the cure of cleft palate. When I have determined that I have a case suitable for operation, I put my patient in regular training, causing him frequently during the day to open the mouth wide, and keep it so as long as possible without fatigue, and by manipulation, familiarizing, and, if I may so express it, reconciling, the fauces, uvula, and back of the tongue to the contact of instruments. During this time I endeavour to improve the general health of the individual by attention to the secretions, and by giving a full allowance of the most nutritious diet. A high condition having been attained, and the person free from cough or irritability of stomach, I usually proceed to operate about mid-day, and two or three hours after the patient has taken a full meal. Shortly before commencing I exhibit a couple of ounces of good

wine, as also during the operation, I may remark, to sustain the sufferer under a painful and exhausting trial.

Having made all my arrangements, I place the patient in a clear light, with the head supported against the breast of an assistant. I then draw the uvula a little forward, and towards the central line, and with a strong, curved, and blunt-pointed scissors, divide the palato-pharyngeus on either side; this I do so deeply as to obliterate completely the posterior pillars of the fauces. I next take Mr. Fergusson's curved knife in my right hand, and having placed the left index finger on the site of the hamular process, and behind the last molar tooth, I pass the instrument in above the velum, on the right side, and sink its cutting point into the pterygoid space by depressing the handle; the stem of the knife-blade then rests against the inner side of the point of the index finger, and the cutting part is about three-fourths of an inch beyond it; then, by giving the knife a slight lateral motion, I cause its point to divide the levator palati, and, probably, some portion of the tensor: changing the knife into the left hand, and using the right index as a guide and fulcrum, I divide the muscles on the left side. There must now be a pause in the operation, as the patient not only requires rest, but, from the hemorrhage, is obliged to gargle the throat freely with cold water. The bleeding having ceased, I seize the uvula of the left side with a curved, sharp-pointed, spring forceps, a little to the outer side of its point, and, drawing it down so as to make the velum tense, I pass a double-edged, narrow-bladed knife through the substance of the latter, opposite the base of the uvula, and about a sixth of an inch from the edge; having first divided the uvula perpendicularly through its point, I carry the knife by a steady cutting motion along the velum, keeping the proper distance from its margin throughout, till it arrives within a line of the apex of the fissure. I next treat the right division of the velum in a similar manner, and then, seizing together the nearly detached portions of the velum which are hanging loosely down, I complete the section by sweeping the knife carefully around the apex of the gap, and in this way I am quite certain that every portion of the margin of the cleft has been duly removed, and to the proper extent.

The next step is the insertion of the sutures. Having two or three curved needles, either mounted in handles or attached to a holder, and threaded, some with a single, and some with a double ligature, 16 inches in length, and of strong white sewing-silk, well waxed, I take one with a double thread, and pass it

through the entire substance of the velum on its left division, about a third of an inch from the apex of the gap, and one-sixth from its margin; having seized the loop of the ligature, and removing the needle, I draw the thread gently forward and give it to the assistant to hold, while I pass a needle, with a single thread, through the right division of the velum in a similar manner. I then take the single thread and attach it by a slip-knot to the loop of the double suture, and by this means draw the latter through the right side of the soft palate; the assistant then takes charge of both portions of the suture, and retains them along the sides of the face of the patient. I pass the successive ligatures in a similar way, leaving about the third of an inch between each. Having introduced the number of sutures I deem requisite, I proceed to make them fast in the following manner:—I take one of the ligatures, usually the upper first, from the assistant, and pass a single end of the one side through the loop on the other; then, by drawing gently on the separate ends of the thread, the cut surfaces are brought together accurately, and without any risk of cutting through the structure of the velum. I next cast on a single slip-knot, and observe whether the edges of the wound are in true apposition, and that the part included within the suture is not compressed by the thread being too tightly drawn. If I discover that the suture is as I desire, I make it fast with a "reef-knot," and cut off the ends. And thus I proceed until all the sutures have been secured. Should any one of the threads appear too tight before being finally fastened, I slacken it, and quite easily, as the loop of the double ligature renders with the greatest facility. Attention to this point is, in my opinion, of great practical importance, for, should the threads be drawn too tightly, there is great risk, when the parts swell, that the sutures may cut their way out, or that the healing process shall fail from the excessive inflammation likely to be produced. The suture I use is, therefore, I believe, the safest that can be applied.

Having given the patient some rest, and caused him to rinse out the mouth and throat, I desire him to open his jaws fully, while I observe if there be any motion in the soft palate, or any undue tension opposite the insertion of the tensor palati. If any such exist, I pass a double-edged knife, half an inch wide in the blade, and curved a little on the flat, through the velum from the front, inside the hamular process, and close to the bone; I then direct its point outwards, make some cutting motion, and thus divide the tensor palati between the point where it passes around its pulley, and where it expands in the velum,

and also, probably, some fibres of the levator palati that may have remained uncut.

The after treatment should consist in giving, at regular and short intervals, good beef-tea, isinglass dissolved in new milk, and the like, for the four first days: indeed, immediately after the operation, a cup of strong soup and a little wine will greatly revive the patient, and strict silence should be enjoined.

The sutures should be removed on the third, fourth, or fifth day, just as the part may be observed to have united: of course, until union has taken place, they should not be taken out, but when there is evidence of union, I am satisfied that the sooner they are gently cut out after the third day, the better.

If any erysipelatous blush should arise about the wound, the part should be pencilled carefully with a twenty-grain solution of nitrate of silver, but not permitting any to touch the wound itself.

The subsequent training of the voice in speaking is a matter of very great interest and importance; for what is the advantage of the most successful operation, what benefit is there after all the patient's fortitude and suffering, if articulation be not improved? Immediately after the operation, and for several weeks, the improvement in the speech is scarcely observable; and without careful education of the voice, a very long time may elapse before the organs of speech adapt themselves to the new condition of parts. If, however, the surgeon will take the trouble of seeing his patient frequently, and will teach him to pronounce those letters over and over again, in the sounding of which he finds him most imperfect, he will soon observe a marked improvement; then causing him to read or speak aloud, marking the words in which there is any defective articulation for correction, he will have the great gratification, within some months, of listening to a fellow-creature who, by his instrumentality, has regained a most useful organ, of which he had long been deprived through a congenital defect.

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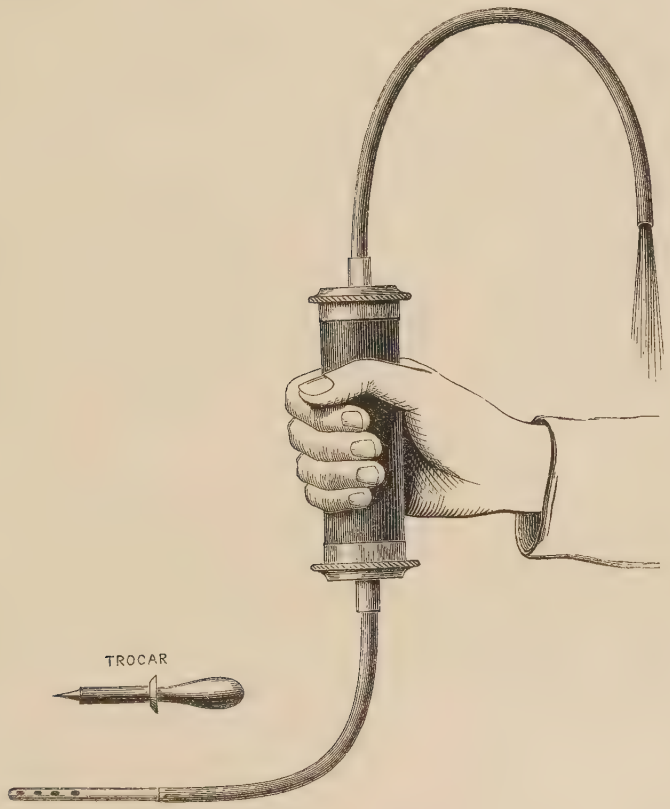
Art. XI.—*A New Instrument for the Operation of Paracentesis.* By Philip Russell, A.B., M.B. T.C.D., Bangor, County of Down.

The instrument which I have invented for the operation of paracentesis consists of—first, a hollow cylinder of India-rubber, about six inches long, and one inch and a quarter in diameter. This cylinder is furnished with two bullet valves—one

at either end—so arranged as to act like a pump when the cylinder is held perpendicularly, and worked by the alternate closing and opening of the hand. To either end of this cylinder is fitted a gum-elastic tube, about twelve inches long, and about the size of a No. 8 catheter. To one of these gum-elastic tubes (viz., that through which the fluid is to pass into the cylinder) is fitted a silver tube, six inches long, and of such caliber as to fit exactly the canula used in the operation. This




tube is an air-tight fit, and passes through the canula just so far as to allow one inch to protrude beyond it, and remain tightly fixed in that position. The free end of this silver tube is rounded like a catheter, and is perforated by three rows of

small holes, to the distance of an inch from its extremity. The other gum-elastic tube affords a passage for the fluid pumped out by the cylinder.

The instrument is used thus:—After perforating the chest with the trocar and canula, the trocar is withdrawn, and the silver tube at once inserted in its place in the canula, and, fitting it tightly, remains there firmly fixed; then, the left hand securing the canula and tube in their place, the right hand, by alternate closing and opening, pumps out the fluid.

This instrument has three advantages which are not combined in any contrivance which has been hitherto used—First, air cannot gain admittance; the two valves effectually prevent this. The moment the hand is relaxed, the bullets close the openings (the cylinder being held perpendicularly), and before removing the instrument, the silver tube should be drawn out of the canula, to the extent of one inch, so as to close the holes at its extremity. As an additional precaution to prevent the entrance of air, the other end of the instrument may be immersed in a basin of water (a suggestion for which I am indebted to Mr. Smyly). Second, the chest can be more completely emptied by means of the pumping apparatus than by the usual method, and it has this advantage over the India-rubber bag and stop-cock, that it need not be removed and reapplied (as the latter must be), and also prevents the admittance of air, which the latter does not effect. Third, the formation of the free end of the silver tube has this advantage over the canula used alone, that, whereas the latter is frequently plugged by lymph, or by the tissue of the lung pressing against it, the former holds back the lung while the fluid escapes through holes in its sides, and as there are three rows of holes, a plug of lymph cannot close them all.

I have used this instrument with the greatest satisfaction in a case of empyema now under my care, the particulars of which I will not now communicate, as the history of a case still under treatment is always unsatisfactory.

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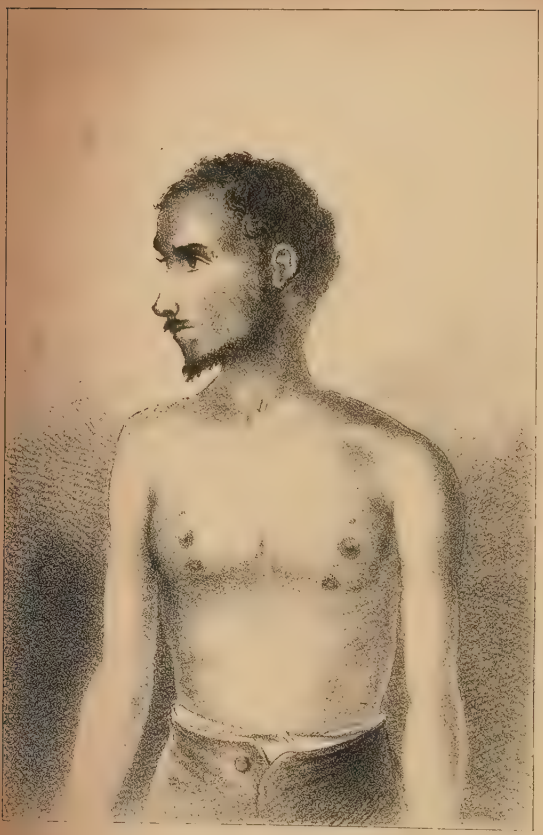

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Art. XII.—*Note of a Case in which a Man had Four Nipples.* By Richard E. Fitzgibbon, Surgeon, 2nd West India Regiment.

The subject of this unusual congenital peculiarity was taken as a recruit by one of my assistant-surgeons, but afterwards rejected by me, in consequence of a delicate constitution, a flat, contracted chest, an old fracture of the right elbow, with partial dislocation of the radius on the humerus, and a shortening (congenital) of the left leg, it being nearly an inch shorter than the right.

His history is as follows:—Daniel M'Corquedale, aged 24, a native of Lucca, in Jamaica, copper-colour, enlisted in the 2nd British Zouaves, 15th July, 1859, but was rejected by a Board on the 29th of August. As is shown in the accompanying drawing, he has two supplementary nipples — one on each breast, about an inch below the regular nipples; there are also two pigmentary deposits, one on each breast, higher up. He states that, when a boy, the supplementary nipples were larger than the natural ones, and that they were a great source of annoyance to him, as, when he went to bathe, the other children used to get about him and keep pulling at *these things;* the pigmentary deposits were also much larger when he was younger than they are now.

This case is, I think, worthy of being placed on record, for although supplementary breasts in the female have been occasionally met with, their occurrence in the male has not been before witnessed, as far as I am aware. The breast in man is usually only rudimentary, although it is occasionally nearly as fully developed as in the female, and instances are not wanting in which it has even secreted milk; but in the present case there was no unusual enlargement of the organ.

The following case is one of the best examples of quadruple mammæ in the female which has been reported. It occurred in the practice of Dr. Robert Lee, and was published in the twenty-first volume of the Medico-Chirurgical Transactions:—

"Mrs. ——, æt. 35, was delivered prematurely of a still-born child on the 21st July, 1835. Soon afterwards, the mammæ became excessively painful and distended, and she had a severe attack of fever with delirium. Though the symptoms became daily more aggravated, a week elapsed before she would permit the condition of the breasts to be ascertained. On inquiring into the cause of this unwillingness to allow the necessary examination of the mammæ to be made, I was informed by

her sister-in-law, that she had two mammæ and two nipples on each side, and that this peculiarity, which she was anxious to conceal, had been observed ten years before, when her first confinement took place.

"After long entreaty, I obtained leave to inspect the breasts, and was surprised to find that there were two on each side, as had been represented; the two of the same side being separated by a deep oblique depression. The inferior or pectoral mammæ, as they were afterwards termed by Sir Astley Cooper, were fully developed and in the natural situation, and their nipples, areolæ, and glands presented nothing unusual in their appearance. Near the anterior margin of the axilla, a little higher up on each side, was situated another mamma, about one-sixth the size of the others. The nipples of these were small and flat, but when gently pressed, a milky fluid, which had all the external characters of the milk secreted by the other breasts, flowed copiously and readily from several ducts which opened on their extremities. When milk was drawn from the lower breasts, a small quantity usually escaped from the nipples of the superior breasts, and when the draught came into the former, the latter invariably became hard and distended.

"Mrs. —— had previously borne several living children, and, five years before this period, had twins, when she had a severe attack of uterine inflammation, and suffered much from painful distention of the two upper breasts. In consequence of the flatness of their nipples, she has never been able to suckle any of her children with these. The vagina, orifice of the uterus, and all the other organs, besides the mammæ, in this female are well formed."

In the paper in which Dr. Lee recorded the above example of supernumerary breasts, he refers to several other cases nearly similar, which had been previously published; but I have not been able to find in the authorities to which I have had access any notice of a similar occurrence in the male.

[In the fifth volume of the present Series of our Journal, Mr. Shannon, Surgeon to the South Dublin Union Workhouse, published an interesting case of quadruple mammæ in a female, and which is illustrated by an engraving; and, in a note by the then Editor, reference is made to an instance occurring in the male on the authority of Dr. Santesson, of Stockholm, which is said to have been published in the Proceedings of the Swedish Medical Society.—Ed.]

Art. XIII.—*On the Prevention of Pitting of the Face in Confluent Small-Pox.* By William Stokes, M.D., Regius Professor of Physic in the University of Dublin, &c.

During the last few years the increased frequency of severe cases of small-pox has given opportunities of testing the comparative merits of the various modes adopted to prevent disfigurement of the face in this disease. These modes may be thus enumerated:—

1. The puncture of the pustules when matured.
2. The application of nitrate of silver.
3. The application of oil, or of the *linimentum calcis.*
4. The covering the face with a solution of gutta percha, with collodion, or with glycerine.

1. As to the first of these modes of treatment, I have no doubt that it tends to lessen the amount of pitting. But it does not appear so applicable to the worst forms of the confluent disease, as to those in which the pustules, though numerous, remain discrete for a longer period than in the severer cases. It is a slow process, but is not very painful, especially if the punctures be made at the right time, and is, I have no doubt, a mode of treatment which may be properly adopted in many instances. I think that, for its successful employment, we should have a benign type of the disease; that the pustules should appear and advance together; and that the amount of inflammation of the skin should not be great. In a case in which I performed puncturing with the greatest care, the crusts remained on the face for a considerable time; pitting was not quite prevented; and the depressions, though extremely slight, were numerous and continued obvious for many years.

2. As to the application of caustic, I have no personal experience, but I have no doubt that the conditions for its successful employment would be nearly those which have been mentioned as essential to the proper use of the first mode of treatment.

3. In employing oil, or the *linimentum calcis*, I prefer the latter, but I do not believe that either can be looked on as of much value. In some cases the heat of the face is so great as to produce rapid drying of any application that may be made. In one very severe case, the oil treatment was diligently pursued, and continued from the first appearance of the pustules until an advanced period of the disease. It was, however, repeatedly observed, that certainly within an hour after the free use of the oil, all traces of the application had disappeared, leaving

the parts hard, hot, and dry, and without any alleviation of local suffering. In this case, a great amount of pitting, especially of the nose and cheeks, resulted. It is worthy of remark that the patient, a young lady, had been carefully and successfully vaccinated when an infant.

It is probable that if the *linimentum calcis* has any advantage, it is owing to the water which it contains, and which acts as a temporary bath or stupe to the part.

4. During the past five years I have used gutta percha and collodion in a considerable number of cases. These, however, were not by any means examples of the worst form of the disease. In most of them the crust came off in large flakes or patches, composed obviously of the dried exudations and the covering material, and leaving the skin uninjured. To render this treatment effective, at least so far as the exclusion of the air is concerned, it is necessary to renew the application at intervals of from twelve to twenty-four hours; for the covering seems to be repeatedly broken up by the advance of the eruption and the swelling of the parts. Some patients are greatly distressed by the feeling of constriction caused by the coating of gutta percha or collodion, and in general the treatment in question appears unadvisable where there is much vascularity, heat, or swelling.

I was first led to adopt this treatment in consequence of the proposal of the late Dr. Graves to produce an artificial cuticle in dealing with various forms of skin disease; see the Dublin Quarterly Journal, August 1, 1852, where may be found reports of two cases of confluent small-pox, in which the solution of gutta percha in chloroform was employed successfully; in both these cases the pain and itching were relieved, and the face continued moist.

They were both of a typhoid character, wine having to be administered freely, and did not, by any means, exhibit the amount of vascularity, heat, and swelling, observable in cases of a more sthenic type.

Dr. Graves advises that the treatment should not be used in the earlier stages of the disease. I believe that this point is not yet to be held as quite settled. It is not proved that the repression of the eruption over a limited space is in chief dangerous; and, as we shall just now see, whatever lessens the vascularity of the skin before the pock shows itself tends to lessen its amount.

I have not employed glycerine, but learn from my friend, Professor Banks, that he has lately employed a combination of glycerine with lapis calaminaris. He speaks highly of its

efficacy as an application to the face. I have no doubt that it will be found a mode of treatment most useful in a large number of cases. Its use is begun from the first periods, and continued until the scabs fall off.

I believe that in reference to the probability of pitting in small-pox, we must take largely into account the character of the disease. If we look at two cases of confluent small-pox, in which the amount of eruption and of confluence are similar, or nearly so, yet with opposite characters of fever, it will be found that the chance of pitting is much greater in the sthenic than in the asthenic or typhoid case. It has happened more than once in the Meath Hospital, that in cases in which, from the condition of the surface after extreme confluence, there was every reason to hope that there would be no pitting, the patient had sunk on the twelfth or fourteenth day. From the rarity of the high inflammatory or sthenic variola during the last ten years, I believe that there are few affections more illustrative of the change in the vital character of disease, so well illustrated by Graves, Alison, and Christison, than that of small-pox. Indeed, most of our hospital cases, for years past, have been of the asthenic type. They showed extreme weakness, a livid hue of the skin, a tardy and irregular advance of the pustules, and an early tendency to confluence. The pulse was rapid and compressible, and the action of the heart, though feeble, was jerking and excited. In the fatal cases the lungs seemed to be deeply engaged, and at about the middle stage of the disease many of the pustules on the legs and feet took on a deep purplish or livid hue, their appearance approaching to that of the vesicles in gangrenous erysipelas.

Looking at the frequency of pitting on the face, as compared with that of other parts of the surface, it is not easy to account for it, unless by referring to the fact that, while the rest of the surface is kept covered, and so not only comparatively excluded from the action of the air, but in a state of humidity, the integuments of the face remain in a dry and heated state—first, from the action of the external air, and next, from the increased vascular action. Hard and hot scabs are formed, and the ulcerative process makes its way downwards to a greater or less degree. Some have held that the liability of the face to markings was to be explained by anatomical considerations. However this may be, it will be found that in cases in which from an early period certain portions of the face have been kept protected from the action of the air, and in a permanently moist state, pitting does not occur. This may be seen in cases of

sthenic confluent small-pox, where, with the view of preventing the adhesion of the eyelids, poultices have been used over the eyes. In such cases it will be often found that every part of the face is marked, except those over which the little poultices had extended.

The application of poultices over the face appears to me to be the surest mode of preventing the consequent disfigurement. We should commence their use at the earliest period, and continue it to an advanced stage of the affection. In most cases they may be applied even over the nose, so as to cover the nostrils; for these passages are generally so obstructed as to be for the time useless to the patient. If the nostrils can be kept pervious by injections, the poultices need not be applied over their orifices.

If the chances of marking are in proportion to the activity of the cutaneous irritation, we may hold that this method should fulfil three important indications of treatment—

1. The exclusion of air.
2. The moderation of the local irritation; and
3. The keeping the parts in a permanently moist state, so as to prevent the drying and hardening of the scabs.

The value of this treatment, however, will, I feel convinced, be best seen in the inflammatory or sthenic form of the disease. The best poultice will be that formed of linseed meal, which should be spread on a soft material, such as French wadding, and covered with the gutta-percha paper or oiled silk. I have never had occasion to repent the adoption of this practice. In a case lately treated in this way in the Meath Hospital the result was very satisfactory, so far as the prevention of pitting over a large portion of the face was concerned. I say a large portion, for on the cheeks thick scabs remained strongly adherent for two months from the first invasion of the disease; they are gradually, however, falling off, leaving the skin uninjured. I have never before seen such tardiness in the falling off of the scabs; and it is a question how far this condition was brought about by the treatment adopted.

The character of this case was typhoid. The eruption came out tardily, and was not well developed until after stimulants had been used; these, too, had to be employed liberally, and for a long period of time.

A remarkable appearance is to be seen in this case when portions of the loosened scab are detached. Long filamentous processes, very similar in appearance to asbestos, are found running from the inner surface of the scab into the skin. I do not think that in this case there will be any pitting, but the

slowness in the detachment of the scab of the cheeks is most remarkable. Alkaline lotions have had little or no effect, and we have lately found the balsam of copaiba, mixed with oil, to be very useful.

In connexion with this subject, I may mention two clinical observations of value:—

Some years since, when the type of small-pox was not so decidedly asthenic, a young woman, who had been always strong and healthy up to the time of her illness, was admitted into the fever wards. She had been two or three days ill, and complained of symptoms which were supposed to be those of an early stage of fever, of an active type. The skin was hot and dry, the pulse strong and full, and the tongue loaded. She had, in addition, a degree of headach much more severe than is ordinarily observed in the commencement of fever. It was, in fact, so intense, that I thought it necessary to use leeches to the forehead in considerable numbers—a practice never adopted by us in the ordinary headach of fever, in which relief is given by three or four leeches, or by warm stupes to the head. In this case about twenty leeches were applied, a great deal of blood was drawn, and much relief followed; but the fever continued, and within the next two days small-pox vesicles appeared on the neck and bosom. The case went on, and proved a most severe example of confluent pock over the trunk and limbs, but on the face not more than two or three aborted pustules made their appearance.

Here the pustular eruption of the face was apparently checked by an early modification of the local vascular action. It is a question how far this view may be held to apply to the following case, already noticed by Dr. Graves in his paper on the treatment of skin diseases:—

A man, who had been under treatment in our surgical wards for a chronic disease of the knee-joint, became affected with fever, followed by the appearance of variola of a severe type. He was transferred to the medical wards, and went through the stages of the disease in its confluent form. His knee had been strapped with mercurial plaster, and at the period of retrocession and the falling of the scabs, the dressings came off. We then observed this singular appearance: above and below the knee, and ending and commencing at a line exactly corresponding to that of the strapping, the integuments were covered with blackened scabs, while the knee remained completely free of any sign of eruption, and silvery white, contrasting strangely with the condition of the parts above and below it.

This case occurred many years ago, and has an additional interest, as bearing on the proposal of M. Briquet to prevent the markings in small-pox by the use of a plaster, in which the principal ingredient is mercurial ointment.

I attribute the results in our case, and in those of M. Briquet, less to any specific action of mercury, than to the influence of pressure on the vessels of the skin[a].

The practical conclusions which I am disposed to arrive at on this matter are as follows:—

1. That the chances of marking are much greater in the sthenic or inflammatory, than in the asthenic or typhoid confluent small-pox.

2. That, considering the change in the character of disease, both essential and local, observed during late years, we may explain the greater frequency of marking in former times.

3. That in the typhoid forms of the disease the treatment of the surface by an artificial covering, such as gutta percha, or by glycerine, will often prove satisfactory.

4. That in the more active or non-typhoid forms, the use of constant poulticing, and of every other method that will lessen local inflammation, seems to be the best mode of preventing the disfigurement of the face.

[a] See Gazette Médicale, 1851.

# PART II.

## REVIEWS AND BIBLIOGRAPHICAL NOTICES.

*Professor Schroeder van der Kolk on the Minute Structure and Functions of the Spinal Cord and Medulla Oblongata, and on the proximate cause and rational treatment of Epilepsy.* Translated from the original by WILLIAM DANIEL MOORE, A. B., M. B., &c. London: The New Sydenham Society. 1859. 8vo, pp. 291.

*Researches on the Intimate Structure of the Brain, Human and Comparative. First Series. On the Structure of the Medulla Oblongata.* By J. LOCKHART CLARKE, Esq., F. R. S. London: Taylor and Francis. 1858. 4to, pp. 28.

THE discovery of new methods of preparing nerve structures for examination in the microscope has led to a fresh series of investigations into the minute anatomy of the nervous centres. Of the investigators into this portion of anatomy, the authors, whose works are before us, are amongst the most notable and successful. For the work of Schroeder van der Kolk we are indebted to the New Sydenham Society, for whom it has been most ably translated by Dr. William Moore, of this city; and, without professing to be learned in the Dutch language, we unhesitatingly pronounce, from the ease with which it reads and the elegance and purity of its diction, that the book could not have fallen into better hands. The author has, moreover, contributed a large amount of new matter, and has altered two or three portions of the original, in reference to which his views have undergone some changes, so that this may virtually be considered a new edition of the essays originally published by the Royal Academy of Sciences at Amsterdam; the first on the spinal cord, in 1854, and the second on the medulla oblongata and epilepsy, in 1858: both of which are here included in one volume.

It is not the least service to medical science conferred by the New Sydenham Society, that they have published this work.

Had it not been for their doing so, the results obtained by Schroeder van der Kolk would have long remained unknown, sealed up by the language in which they were published, for the book is not one that would be likely to pay for its publication in English, as a commercial speculation, and yet it is one whose teachings will, we doubt not, become as "familiar in our mouths as household words."

The second work on our list is a republication from the Philosophical Transactions for 1858, of a most valuable series of papers on the structure of the medulla oblongata, by J. Lockhart Clarke, Esq., already distinguished by his researches on the structures of the spinal cord. Both books are profusely illustrated with well-executed lithographs.

Our knowledge of the structure of the nervous centres has hitherto been founded on a superficial anatomy, deductions from physiological and pathological facts, experiments on living animals, and observations in comparative anatomy. We have now, in some measure, the means of testing the accuracy of the inferences from these sources. The interest and importance of doing so will, we trust, induce our readers to follow us into some details, confining our attention, in the first instance, to Schroeder van der Kolk's Essay on the Spinal Cord.

The teachings in our schools, and in the leading systematic works on physiology, embrace three theories as to the structure and mechanism of the spinal cord, which, following Dr. Todd, we may explain as follows:—First, there is the theory of Dr. Marshall Hall and his followers, which teaches that the spinal cord has two sets of nerve fibres connected with it,—fibres connecting the various muscles and sentient surfaces of the body with the brain, passing along the cord, so that that organ is little more than a bundle of nerves going to and from the brain. These are called *sensori volitional* nerves, as carrying sensation to the brain, and volition from it to the muscles. But, in addition to these, there is, according to Dr. Hall, another class of fibres proper to the spinal cord, which form a connexion with its gray matter; of these fibres, some are afferent or incident, others efferent or reflex, and these two kinds have an immediate, but unknown relation to each other, so that each afferent nerve has its proper efferent one, the former being excitor, the latter motor. This theory was advocated by Dr. Carpenter, in the early editions of his Physiology, who, however, found it necessary, in order to account for certain emotional acts, to suggest the existence of another set of fibres—emotional fibres passing along the spinal cord to the brain. Thus the roots of the nerves were supposed to consist, each of

them, of three sets of fibres: the anterior root, containing 1st, fibres of voluntary motion, arising in the brain, and running along the cord to the point of exit; 2nd, fibres of emotional acts, also arising in the brain, and running along the cord; and 3rd, reflex fibres arising in the gray substance of the cord. In like manner, the posterior root was supposed to contain three sets of fibres, 1st, sensitive, 2nd, emotional, continuing through the cord to the brain, and 3rd, excitor, terminating in the gray substance of the cord itself.

The second hypothesis is that which accords with the views of Müller, and many other physiologists. It assumes that the fibres of sensation and volition proceed to and from some part or parts of the intercranial nervous mass—that every nerve-fibre of the body is continued into the brain. Those which are distributed to the trunk and extremities pass along the spinal cord separating from it, with the various roots of the nerves, and in their course within the spine mingling more or less with the vesicular matter of the cord. There are, according to this hypothesis, no other fibres but these (save the commissural), and they are sufficient to manifest the physical, as well as the mental acts. Nerves of sensation are capable of exciting nerves of motion, which are in their vicinity, and they may produce this effect even when the spinal cord has been severed from the brain, for their relation to the gray matter of the cord is such that their state of excitement is readily conveyed to it.

According to the third hypothesis, first proposed by Todd and Bowman, it is assumed that all the spinal and encephalic nerves, of whatever function, are implanted in the gray matter of the segments of the cerebro-spinal centre, with which they are severally connected, and *do not pass* beyond them. The several segments of the cerebro-spinal axis are connected with each other, through the continuity of the gray matter, from one to another, and through the medium of commissural fibres, which pass between them. Through these means motor or sensitive impulses may be propagated from segment to segment; and a stimulus conveyed to any segment from the periphery may either simultaneously affect the brain and cause a sensation, or it may be reflected on the motor nerves of that segment, and cause their muscles to contract; or both these effects may take place at the same moment, as the result of one and the same stimulus.

According to this hypothesis, each segment of the cord, so long as it retains its proper commissural connexion with the brain (by commissural fibres and continuous gray matter), is part and parcel of the centre of volition, as well as of that of sensation,

and the mind is as directly associated with each segment of the cord, as it is with any portion of the encephalon. Let that commissural connexion be dissolved, and the mind will immediately lose its hold upon the cord; but the various segments of that organ may, nevertheless, be still acted on by the physical impulses, and may still continue to evolve the nervous force in connexion with the natural changes which may take place within.

Dr. Todd, from whose article on the Physiology of the Nervous System in the Cyclopædia we have mainly taken the foregoing description, supports the latter theory by many anatomical, physiological, and pathological facts, of such weight that the theory is pretty generally adopted. We propose now to subject it to a most severe test, viz.—to compare the inferences drawn by Dr. Todd from his various facts as to the structure of the cord, with the facts actually ascertained by microscopic observation.

The point of variance in the three theories, it will be observed, is as to whether all the fibres of the spinal nerves terminate in the cord (Todd and Bowman), or whether all continue along it to the brain (Müller), or whether part terminate in the cord, and part continue on to the brain (Hall).

Schroeder van der Kolk has found that the gray substance of the spinal cord is composed chiefly of ganglionic corpuscles of the multipolar variety,—that is, cells, such as we lately described in our review of Drummond on the Sympathetic Nerve, having numerous processes. The fibres composing the anterior roots of the spinal nerves he describes as penetrating to these corpuscles, and becoming continuous with some of their processes; some corpuscles having two or more nerve processes connected with them, some but one; where there are two or more fibres arising from one corpuscle, they often pass out in different parts of the cord, joining different nerves, and the fibres from the several nerves frequently pass over neighbouring corpuscles to become connected with distant ones. The corpuscles are arranged, more or less, in groups, and are connected with one another by their processes, and the several groups are similarly connected. Schroeder van der Kolk believes that none of the fibres of the anterior roots become continuous with the longitudinal fibres of the spinal cord; the latter he regards as commissural bands connecting the several groups of corpuscles with one another, and with the brain. The connexion being established by certain processes from the multipolar corpuscles, that he calls transverse fibres, which pass out between the longitudinal fibres, and become connected with them.

"We see this represented[a] in Fig. 4, only a few longitudinal medullary fibres, *a a*, being retained, in order to render the drawing not unnecessarily large. We here observe inferiorly, under 1, not less than eight filaments in connexion with ganglionic cells, while from the uppermost bundle four filaments can be traced to such bodies. Among these filaments those marked 1 1′ are both derived from the same ganglionic cell, the anterior in the centre, which gives off a filament to each fasciculus; at 2, we see two filaments, derived from the one end of a ganglionic cell, while the cell marked 4, likewise gives off two filaments, running out in an oblique direction under the other nerve filaments to the superior bundle at 3, just as the cell 5 likewise gives off a nerve filament, which passes obliquely over another ganglionic cell running near 1′. At 6 is the most remote ganglionic cell, connected with the nerve filaments; but from the fasciculus at 7, which is not traced, a filament was connected with a cell situated at a still greater distance, but which I have omitted, in order not to enlarge the drawing unnecessarily. The connexion of numerous ganglia with one another (as the cell 8, and the cells marked with 9) is here also very beautifully shown. Most of the connecting filaments of these cells are, however, cut off, while many filaments, visible in the object, but which could not be traced, and which would only have made the drawing indistinct, are not included; as it is impossible to reproduce in a drawing the entire copiousness of nature. In several places, moreover, in this very successful preparation the passage of the nerve filaments into the ganglionic cells was clear to demonstration; the specimen was taken from the lumbar portion of a cow. The origin of the anterior nerve-roots from the ganglionic cells is here so evident as to leave no room for doubt on the subject. . . .

"There can therefore be no doubt that the roots of the motor nerves arise from the spinal cord, and more particularly from the ganglionic cells of the anterior horns, which unite to form a network, and often divide into more or less widely separated groups.

"The principal question therefore is: in what manner are these nerve-roots connected, by means of the ganglionic network into which they pass, with the brain?

"That the anterior medullary filaments, which are represented in Fig. 5, as cut across at A A A, are the conductors of the orders of our will to the motor nerves, has been fully ascertained, and is admitted by all physiologists. The connexion stated to exist between these medullary filaments and the gray matter is, however, not so evident. In order to demonstrate this connexion, it is necessary to turn our attention to some other parts of the spinal cord, which, in my opinion, are immediately concerned in it; namely, the

[a] Van der Kolk's work has been so extensively circulated by the flourishing Society which has printed it, that we believe most of our readers will have it by them to refer to the plates. The description here is, however, sufficiently plain to be understood without the plates.

transverse fibres, found on all sides among the medullary longitudinal fibres, and radiating from the gray substance through the white. See Fig. 12, *f, g, h.*"

The connexion of the transverse fibres with the longitudinal is difficult to demonstrate, but Schroeder van der Kolk has succeeded in this, and has given a drawing of it. Speaking of these fibres, he says:—

"I have, however, observed, that they curve and pass into the longitudinal fibres, as I have represented in Fig. 6, from a very successful longitudinal section, taken from the antero-lateral columns. We here see the transverse strands, *c d*, bending into the longitudinal fibres; yet at the same time the innermost longitudinal fibres curve towards the gray substance and pass into the ganglionic cells at *a g*, a fact which Schilling also represents, and says he has only once observed. I have often met with this, and in the preparation whence Fig. 6 was taken, only a part of which is represented, it occurred in several places. . . .

"From the foregoing, the nature of these filaments may, without much difficulty, be explained. Thus the longitudinal white columns, for the most part, maintain, as all writers have observed, parallel courses, and present no apparent termination, while from the gray substance lateral transverse bundles radiate, which divide and are scattered among the white columns and take up their fibres, Fig. 6, *c e d f*. In this manner the longitudinal fibres, as conductors of our will, pass into these transverse fibres, and thus the orders of our will are conveyed along these rays to the ganglionic net, whence the motor nerves take their origin. This of itself explains why so many ganglionic cells are situated at the outer edge of the gray matter, into which we do not see the nerve-roots pass; these last arise rather from the centre of the anterior horn, namely, from the ganglionic group, which receives on all sides communicating filaments, arising themselves from the marginal ganglia or from the transverse rays.

"It is, however, not always that such a decussation of the transverse radiating fibres with the nerve-root at its entrance is seen; sometimes the filaments of the nerve-root themselves appear in part to bend along the margin of the gray substance, and here to pass into cells which are connected with the radiating fibres, while other fibres keep more to the middle and penetrate to the principal group of the ganglionic cells.

"These transverse radiating fibres are, therefore, the means of communication between the anterior and lateral longitudinal columns and the gray substance, or rather the ganglionic cells, whence the nerve-roots arise. Hence is seen also why the longitudinal fibres of these white columns maintain such a parallel course. They would of necessity exhibit a more plaited appearance, if fasciculi of some of the fibres constantly passed from without between the others to-

wards the centre in the gray substance; but it appears, on the contrary, that the gray matter sends off transverse rays, subdividing among the longitudinal fibres, and taking up these fibres, or, if we will, passing into them. It also follows from this, that these rays are most numerous where the thickest nerves arise, or where the most complex nervous connexions are situated; to be convinced of this it will be sufficient to compare the first three drawings in Stilling's work on the medulla oblongata, for example, Plate II., Fig. 1, from the dorsal portion, with Figs. 3 and 4 from the cervical bulb. I cannot, however, describe this connexion very fully, until I come to the physiological explanation of this wonderful tissue of fibres in the spinal cord, to which I refer the reader, but as preparatory to which I must first explain the structure of the spinal cord and of its tissue in its integrity."

It is thus seen that microscopic observation has fully demonstrated the correctness of the theory of Todd and Bowman, so far as the motor roots are concerned; and here we must observe, that we think Schroeder van der Kolk, as well as Brown-Sequard, has not done justice to our countrymen, who have so remarkably arrived, by reasoning from physiological facts, at a knowledge of the structure, since demonstrated by the microscope. We are accustomed to regard the fulfilment of the predictions of astronomers as wonderful proofs of the accuracy of their science, and on the same grounds we must now congratulate Todd and Bowman, and the students of physiology, on this conclusive proof of the accuracy to which their science also may attain.

In reference to the sensitive or posterior roots of the spinal nerves, the microscope seems to have not yet decidedly pronounced, owing, in a great degree, to the complicated structure of the parts. Here we must again, for the sake of clearness, digress somewhat from our author. It is well known that after Sir Charles Bell proved the difference in the functions of the anterior and posterior roots of the spinal nerves, he believed that the anterior and posterior columns of the spinal cord were devoted, respectively, to motion and sensation in the same manner as the nerve roots arising from them. Sir Charles soon afterwards gave up this theory, but it has since met such warm advocates, especially Longet, that it has been very generally taught in the schools and in the class-books. Todd and Bowman examined this question with their usual ability, and arrived at the conclusion, that the posterior columns are not concerned in conveying sensitive impressions to the brain, that they have no connexion with the posterior roots of the spinal nerves, which are attached exclusively to the antero-lateral co-

lumns, and the gray substance in them, and that these antero-lateral columns convey both the motor and sensitive impressions to and from the brain; while the posterior columns are exclusively commissural, connecting the several segments of the cord with one another, and with the cerebellum, and that they are principally of use in harmonizing and co-ordinating the actions of the several parts of the cerebro-spinal axis. A great part of the lectures lately delivered by Brown-Sequard in London and Dublin, and many of the experiments he performed, were devoted to the examination of these same posterior columns. The conclusions at which he arrived coincided with those of Todd and Bowman, so far as that they took no part in conveying sensitive impressions to the brain, which Brown-Sequard believes are conveyed by the gray substance, and more particularly by its posterior horns, while the office of the posterior column is chiefly, he allows it to be inferred, though he does not distinctly state it, to supply nerves to the blood-vessels—vaso-motor nerves—a theory we are not called on to discuss here, and one not without its difficulties. They also, he says, take part somehow in the reflex functions of the cord. To us his reasonings by no means disprove the theory of Todd and Bowman as to the commissural nature of these columns, but rather the contrary; but it concerns our present subject more to notice the interesting discovery of Brown-Sequard, of reasons to believe that the conductors of sensation from the two sides of the body decussate with one another at the point of their entrance into the spinal cord; that they all join the central gray substance, but that in doing so, some of the fibres pass between the longitudinal fibres of the posterior and antero-lateral columns, without, however, being connected with them, and that, before joining the gray substance, some of them run a short distance upwards, and some downwards, while the others pass in directly to their destination. We may now return to the work before us, to learn the teachings of the microscope on these points.

The examination is attended with great difficulty, which will not only account for the various opinions of different writers, but for the readiness with which Schroeder van der Kolk has changed his own opinions since the first publication of his essay, as appears not only from the notes he has supplied to this edition, but from his more recent essay on the Medulla Oblongata. His first observations led him to believe that the posterior roots divided into two sets on joining the cord, some passing upwards, forming a part of the posterior columns, and thus passing to the brain; while others pass directly in to join the

multipolar cells. The first set he believed to be for sensation, and the others to be reflex nerves.

"From the foregoing it would therefore appear, that posteriorly not only is the tissue of the spinal cord more complicated, but contains two kinds of nerve-roots, one kind proceeding immediately upwards in the white medullary rays, Fig. 7 *a b*, and apparently passing to the brain. They are indisputably nerves of sensation; the other roots, however, Fig. 7 *c d*, Fig. 8, *a*, *b*, *c*, pass across through the white columns to the posterior horn, which they penetrate, and partly mingle with the marginal fibres, by which the horn is surrounded, and, as it were, enclosed, and which, in the centre of the gray substance between the anterior and posterior horn appear to lose themselves in ganglionic cells, Fig. 8 *d e*, wherein at the same time both the major part of the marginal fibres and the posterior commissure, *f*, unite. These nerve filaments can scarcely be anything else than reflex nerves, conveying the stimulus into the network of ganglionic cells, wherewith they appear to be connected, whence the impression received can be communicated to the anterior groups of cells, from which the motor nerve-roots arise. These transverse roots, which have hitherto been regarded as sensitive roots, appear therefore to be reflex roots, which accompany the sensitive nerves into the spinal cord, where they leave one another; the first to pass upwards to the brain, the others to unite with the common central point for motion, that is, with one or other group of ganglionic cells in the commencement of the anterior horns. The posterior horn itself, which, on account of its great transparency, is stated, although improperly, to consist of gelatinous matter, has, on a transverse section, an elegant and wavy appearance, Fig. 8 *g g*, being very transparent towards the ends, with a dark marbled border. Several bundles, appearing as darker striæ, perforate this horn; its structure, therefore, differs very much from that of the anterior horn, which has a more uniform tint, and is more or less occupied with ganglionic cells, and the many decussating filaments, springing from them."

This certainly is a very different structure from that which we might expect to find on the authority of Todd; but the author has, since the publication of the essay, materially altered his opinion, as will be seen by the following extract from his essay on the Medulla Oblongata. It is, morever, to be observed that Bidder has failed to discover those ascending fibres; and that Stilling, who has devoted so much attention to the examination of the spinal cord, while he confirms their existence, describes all the fibres of the posterior roots as joining the gray substance in its posterior horns, some passing in at once to their destination, and others running upwards some distance, and terminating in the gray substance, which exactly accords with the teachings of physiology.

"From the great analogy, however, which exists between the sensitive nerves of the spinal cord and of the medulla oblongata, I think we must for the present infer, that the nerves of sensation in the cord also arise from the ganglionic cells in the posterior gray cornua, whence, probably, filaments are derived, which decussate in the commissures and pass farther upwards. Such a view would afford the best explanation of Brown-Sequard's experiments, and would reconcile them with the indisputable facts of the course of the sensory nerves in the medulla oblongata. From these experiments it would appear to follow as a necessary consequence, that the decussation for the nerves of sensation takes place in the spinal cord, in the neighbourhood of the origin of the sensory root. . . .

"As, however, the posterior grey cornua, as I have observed in my Essay on the Spinal Cord, pages 52 and 68, are much more slender in the back and above the cervical bulb, and therefore contain a much smaller number of the fine ascending fibres than exist in the lumbar and cervical expansions, these fibres cannot, in my opinion, serve for the conduction of perception; on the contrary, while the proper sensory filaments appear to pass over into the ganglionic cells of the spinal cord, other conducting filaments spring from this origin of the nerves of sensation, which, as in the medulla oblongata, seem to pass to the opposite side, to turn upwards into the posterior columns, and so to convey the impression to the brain. Hence it also follows, that we cause insensibility, according to the experiments of Brown-Sequard, by a longitudinal incision into the middle, or by destroying the gray matter. Brown-Sequard appears, however, not to have, in every instance, accurately distinguished reflex impressions from the perception of feeling."—*Essay on Medulla Oblongata*, pp. 114.

*Physiological results* having proved that the motor nerves take their origin from the multipolar cells in the spinal cord, and that these cells are arranged in groups, the author proceeds to explain the mode of action of these parts—

"If we examine a muscle, the biceps for example, or any other muscle of some size, and trace the motor nerves distributed to it, the slightest microscopic examination will suffice to show that the branches of the so-called perforans Casserii, which pass into the biceps, contain several hundreds, nay, thousands of primitive filaments; from the same nerve the brachialis anticus also receives its motor nerves, whereby this number is again considerably increased. The peculiarity, however, of these muscles is, that we cannot move either separately; they form one of those systems, of which some still more extensive examples are to be found in the body; it will suffice to mention the rectus, cruræus and vasti. When we bring the first-mentioned muscles into action, all the fibres belonging to the system to which they belong become proportionately tense; we are not able to flex the biceps and to leave the brachialis anticus relaxed, or

*vice versa;* but whether it be with slight force, or whether it be with a violent effort, the influence of our will is proportionately communicated to all the muscular fibres,—a co-operation absolutely necessary to enable the muscles to render us the service of which they are capable. The power of producing a partial contraction of some muscular bundles would have been perfectly useless; because, in these muscles a part could have no more action than the entire organ, namely, to flex the forearm; the orders of our will must, therefore, be uniformly distributed over all the muscular nerves entering into the brachialis anticus and biceps. Now, if all these muscular nerves proceed along the spinal cord to the brain, and, in fact, to the point where our will acts, the latter must always uniformly influence these thousands of nerve-fibres; but still it remains unexplained, why our will should not be able to act more strongly on some nerve-fibres than on others, as it can act on different muscular nerves, and why we should not have the power, for example, of bringing into action the one-half of the biceps, or the biceps alone, leaving the brachialis anticus relaxed.

"Nature has, however, in my opinion, arranged this with much greater simplicity and certainty. All the muscular nerves, destined for the biceps and brachialis anticus, arise from a group of multipolar ganglionic cells, which are mutually connected. If now this group receives an impression or stimulus from our will, by means of one or several conducting filaments derived from the brain, this impression appears to be distributed equally to, and to excite to the development of force, all the multipolar cells of the group, by means of numerous filaments of communication; the consequence of which is, that all the nerve fibres derived from the group are excited with an uniform force, which is transmitted to all muscular fibres receiving nerves from the same group, whence must result an uniform and simultaneous action and contraction of all muscular fibres of the biceps and brachialis anticus, so that we are unable during their contraction to leave a portion of these muscles inactive. We may, therefore, regard such a group of ganglionic cells as a battery of Leyden jars connected with one another; the electric force is divided uniformly over all the united jars, and all are at the same time uniformly discharged, as a conductor alone is necessary to enable all to be charged alike."

It follows, then, that the number of groups of cells will correspond to the number of groups of muscles, and that the number of longitudinal fibres in the spinal cord, required to conduct volition from the brain, will be in proportion to the number of groups of cells; and that the harmonization of muscular movements depends on the muscles being connected with special groups of cells, by the stimulation of which a combined or co-ordinated movement takes place, such as we see in a jump or a step; shrieking is, according to our author, but one of these

harmonized reflex movements. Hence, it follows, he says, that in vivisections so many incorrect inferences are drawn as to the feeling and perception of animals. If the brain be cut off above the pons varolii, and the fifth pair of nerves be strongly stimulated, the animal will cry out, although without consciousness, without perception, and without the feeling of pain; and many instances of shrieking in the human subject are referred to, where there was no trace of consciousness.

2. We now turn to the consideration of the medulla oblongata, treated of in Van der Kolk's second essay, and in Clarke's paper. The mode of treatment by the two authors is essentially different, Clarke confining himself almost completely to anatomical details, which are plainly and clearly put forth, and well represented in the drawings, while Van der Kolk's essay is more physiological than anatomical, and, we must say, does not seem to us to be of the same value as his essay on the spinal cord. The whole argument, in fact, is founded on certain premises which are at variance with the teachings of other observers; and as Van der Kolk assumes them, rather than proves them, they cannot be admitted. These are, 1st, that the posterior columns of the spinal cord are sensational, which, as we have seen, the observations of Todd and the experiments of Brown-Sequard disprove, and also that the microscopical researches of Bidder and Stilling confirm the views of these authors. 2nd. It is assumed, on the strength of certain experiments of Schiff, that the anterior white columns of the spinal cord are the conductors of the orders of the will for the movement of the extremities, and the lateral columns for the movement of the trunk; a theory at variance with the result of Brown Sequard's experiments, and the generally received doctrine. 3rd. Van der Kolk states that—

"Of the spinal cord, only the anterior columns pass, in the corpora pyramidalia, as conductors of the orders of our will for the movement of the extremities upwards towards the brain. The lateral columns of the cord terminate on a level with the vagus, which is intimately connected with, and exercises a reflex action on them. Consequently, in hemiplegia we never have paralysis of the half of the trunk, but only of the face, the tongue, and the extremities."

This is the second conclusion he draws in summing up his observations, and it is so completely at variance with the universally received doctrine, that it should have been fully discussed, which it is not. On this point we find that Clarke has almost completely confirmed the universally received opinions,

and leaves no doubt as to the inaccuracy of van der Kolk as to the abrupt termination of the lateral columns,—a statement entirely new to us, as well as to his revival of the error, long since corrected by Rosenthal, that the anterior pyramids are formed by the anterior columns of the spinal cord, the fact being that they are formed chiefly by the lateral columns.

"In addition to the fibres that proceed from the lateral columns and posterior gray substance, there are other decussating fibres that enter into the composition of the anterior pyramids. These are derived from the anterior gray substance, and belong to the system which constitutes the anterior commissure of the *medulla spinalis*. They are less numerous, however, and most distinctly seen at the lower parts of the pyramids.

"The anterior pyramids, therefore, are composed of four orders of fibres:—

"1. Decussating fibres from the lateral columns, forming their chief bulk.

"2. Decussating fibres from the posterior columns and posterior gray substance, particularly at their upper portion.

"3. Decussating fibres from the anterior commissure.

"4. Non-decussating fibres of the anterior columns, separate on their outer side, and on their inner side incorporated with those that form the decussation."—*Clarke*, pp. 14, 15.

While we object thus to van der Kolk's physiology, we gratefully acknowledge the great value of his anatomical observations, and especially the further proof he affords of the law that nerves arise from special groups of multipolar ganglionic cells, which groups are connected with one another, and with other centres, by commissural bands. Whether he is justified in arguing that these commissural bands establish a crossed action in the nerves that arise above the decussation of the pyramids, similar to that admitted to occur in nerves rising below this decussation, will, probably, still be doubted. The argument is chiefly founded on the position of the tongue in cases of paralysis. The tongue is commonly believed to be protruded obliquely in cases of paralysis, the tip being pointed *in general* towards the paralyzed side. This, it is well known, is not a constant occurrence, but its coincidence with hemiplegia is regarded by van der Kolk as proving a decussation in the action of the hypoglossal nerve. It is remarkable that anatomists cannot agree as to the mechanism by which this supposed obliquity is produced; and in the attempts to account for it, the most incongruous and contradictory actions are attributed to the muscles of the tongue, of which examples are afforded in the various theories quoted by our author. This symptom, obliquity

of the tongue, is one that has long occupied our attention, and in all the cases we have seen where it was supposed to exist, we have shown that the obliquity was only *apparent*, the tongue being really protruded in a straight line, its centre corresponding to the division between the two anterior incisor teeth, but from the *lips being drawn to one side*, the tip of the tongue *appears* to be directed to the paralyzed side. We believe that paralysis of centric origin is rarely or never seen in the nerves arising from the medulla oblongata, because disease affecting this centre interferes with respiration, and causes rapid death. Even of the seventh nerve, Dr. Todd says, in his Clinical Lectures, he never saw it completely paralyzed from uncomplicated disease of the brain; intercranial disease may, as in the case of cancerous tumour described by Watson, cause paralysis of the nerve, and, in the same way, intercranial disease may paralyze the lingual nerve; but in this case we believe the tongue would be protruded towards the sound side by the action of the styloglossus and hyoglossus, and the paralysis would be on the same side as the intercranial tumour. At all events, we do not think it is yet proved that there is any crossed action in the nerves arising from the medulla oblongata. We further object to the limitation of the seat of perception to the medulla oblongata. Todd, in the article on the Physiology of the Nervous Centres in the Cyclopædia, so often referred to, argues that the seat of perception is in the optic thalami, corpora quadrigemina, and medulla oblongata, which we believe to be much more in consistence with anatomical facts; but it may be that van der Kolk includes these parts under the term medulla oblongata, as some other authors do, a point on which he leaves us in doubt, as he gives no definition of the term. Nor can we regard the theory that the corpora olivaria specially preside over the function of speech and the complicated movements of the tongue, as proved, though the intimate connexion of these bodies with the nuclei of the hypoglossal nerves lends probability to the suggestion. The researches of Clarke, however, show that these connexions are not more intimate than those of these bodies with all the other parts of the medulla oblongata, and, on the whole, we believe that anatomy will not justify our going further than he does in the following extract. As for van der Kolk's cases, the post-mortem examinations showed such an amount of concurrent disease as to prevent any argument being safely rested on them.

"The olivary bodies, then, are intimately connected with all the surrounding parts of the medulla, chiefly through the arciform sys-

tem, as will be more fully shown further on; but I have never been able to trace any immediate connexion between the cells of the lamina and the roots of the nerves,—not even of the hypoglossal nerves which pass directly through it. It is probable, therefore, that the *olivary bodies* are co-ordinating centres for the different ganglia or nuclei of the *medulla oblongata.*"— *Clarke*, p. 29.

In leaving this part of our subject, we must express our opinion, that the patient investigation of the strictly anatomical relations of the several parts of the medulla oblongata pursued by Mr. Clarke has given us more insight into its structure, and affords better promise of arriving at a knowledge of its physiology than any thing in the more pretentious essay of the Dutch professor. The plan pursued by Mr. Clarke is, no doubt, tedious, and demands patience; but of its value as a means of ascertaining the arrangements of a complicated and intricate structure, his essay affords full evidence.

"It is well known that in the *spinal cord* the columns of white and gray substance are parallel to each other, and preserve the same relative position throughout their entire course; but in the *medulla oblongata* these parts not only assume a different arrangement, by becoming more or less blended with each other and with new structures, but frequently pursue a curvilinear direction in different planes inclined at varying angles. It is evident, therefore, that transverse and longitudinal sections, however transparent they may be made, must be insufficient, when employed alone, to complete the examination of so intricate a structure. For this reason, after having made myself thoroughly acquainted with the microscopical anatomy of these parts, and traced their relations and connexions, as far as possible, by my own method of preparation, which I find superior to any other that I have seen of the same kind, I then employed, in conjunction with it, the method of Reil; so that, when necessary, the structure revealed at different stages of the dissection was compared with transparent sections through the same parts. Great assistance was also obtained by making, simply for my own use, as I dissected through the depth of a part, a multitude of sketches, which were placed side by side and carefully examined in succession: the plan is tedious and demands patience, but can be confidently recommended to anatomists as a valuable expedient for ascertaining the arrangements of a complicated and intricate structure, of which the parts are so continually changing their course and relative position, in varying planes, that in the dissection each must necessarily be destroyed whilst seeking its relation to others."—*Clarke*, p. 2.

III. *Pathological investigation of the Medulla Oblongata in general, and especially of the proximate cause and rational treatment of Epilepsy.*—This forms the second part of van der Kolk's essay on the Medulla Oblongata, and is in every respect

more valuable than the first. His physiological researches had early led him to regard the medulla oblongata as the principal centre whence the more general reflex movement and convulsions derive their origin. Even though the primary irritation may be remote—for example, in the intestines—a morbidly elevated sensibility and irritation in the medulla oblongata always form, he was led to believe the foundation of such attacks. Influenced by these views, he applied himself to the steady examination of the subject. Many interesting facts were thus brought to light, which are here detailed: among others, it was discovered that the medulla oblongata, in cases of long standing, presented certain appearances of the effusion of lymph into its substance, and, at a later stage, fatty degeneration. It was also found that the capillaries became permanently enlarged, and that the seat of this enlargement varied according as the patients bit their tongues during the paroxysm, or were chiefly affected with difficulty of breathing; the enlargement, in the first case, being found in the neighbourhood of the nucleus of the hypoglossal nerve, and, in the second, in the nucleus of the par vagum.

"From all this, I think we have sufficient reason to conclude, that the first cause of epilepsy consists in an exalted sensibility and excitability of the medulla oblongata, rendering the latter liable to discharge itself on the application of several irritants which excite it, in involuntary reflex movements. This irritation may either be external (irritation of the trigeminus), an irritated condition of the brain, or, as is still more frequent, it may proceed from irritants in the intestines. In children, worms in the intestines, acidity, a torpid state of the bowels, &c., are among the most common causes; in adults, there may be irritation of the intestines, particularly of the mucous membrane, constipation, and prolongations of the colon connected therewith, but above all, onanism, which acts so very much on the medulla oblongata, and must be regarded as a very frequent cause of epilepsy. Amenorrhœa, chlorosis, plethora of the uterus, hysteria, &c., are also to be enumerated.

"In the commencement there is still only exalted sensibility. If this can be removed or moderated, the epilepsy gives way of itself, especially if the sensibility is not renewed by remote causes.

"But if the disease has already lasted long, organic vascular dilatation takes place in the medulla, the consequence being that too much blood is supplied, and the ganglionic groups are too strongly irritated, too quickly overcharged. Every attack then becomes a renewed cause of a subsequent attack, as the vascular dilatation is afresh promoted by every fit. Lastly, increased exudation of albumen ensues from the now constantly distended vessels, whose walls at the same time become thickened, producing increased hardness of

the medulla, subsequently passing into fatty degeneration and softening, and rendering the patient incurable.

"With these changes, vascular dilatation in the brain, and particularly in the cortical substance, goes hand in hand. The small ganglionic cells, which are here present in such great abundance, become compressed by the dilated vessels, and perhaps also in consequence of the more albuminous nature of the intercellular fluid. Dulness and loss of memory are the results; or if after a fit an unusual current of arterial blood is supplied, we have, following immediately upon the paroxysm, over-irritation, rage, and acute mania, which is present in so many epileptics.

"This dulness of the mental powers, which may represent apparent, until it finally passes into true dementia, differs very much from dementia after acute or chronic mania—a point which is not sufficiently kept in view. Dementia after epilepsy is for a long time the result of vascular dilatation, and, as I shall show, if we succeed in conquering the epilepsy, the silliness, the blunting of the mental powers, and the shortness of memory give way, and the patient gradually regains his former powers of mind, although not always in a perfect degree, which depends on the amount of the affection. The dilatation of the vessels, kept up by no fresh attacks and congestions, begins by degrees to disappear, the vessels contract, regain their tone, the albuminous exudation becomes absorbed, and the patient recovers."

There is a weak point in this theory that must at once occur to all our readers, viz., that it does not account for the unconsciousness which is the characteristic feature of epilepsy, and not unfrequently the only symptom of the disease. Van der Kolk does not regard it as essential, but rather as a secondary phenomenon, produced by the spasmodic contraction of the blood-vessels of the brain, arising from an impulse reflected from the medulla oblongata. The theory is virtually the same as that advocated by Brown-Sequard, and his experiments are brought forward in support of it; but we observe that this experimentalist, in his lectures, following Kussmaul and Tenner, places the contraction of the cerebral blood-vessels, and the subsequent loss of consciousness, as the first consequence of the reflex action of the over-excited medulla oblongata, which has been called into play in consequence of the stimulus conveyed to it from some remote irritation.

The treatment recommended by the author resolves itself into two principles, of which, strangely enough, he places first, to remove the over-sensibility of the medulla oblongata; and second, to remove the local irritation, as worms, &c. After trying many means to accomplish the first object, he came to the conclusion that it could be best effected by local depletion,

as by leeches and cupping to the back of the neck, and by deep counter-irritation, by setons, issues, and the actual cautery.

It is almost needless we should remark, that however useful these means may be in many cases, there is a numerous class of cases in which a tonic and stimulating treatment will alone be attended with benefit.

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1. *Om nervernas regeneration och dermed sammanhängande förändringar af nervrören.* Akademisk af handling af Dr. Otto Edvard August Hjelt. Helsingfors: J. C. Frenckell och Son. 1859. 8vo, pp. 72, with plates.

   *On the Regeneration of Nerves, and the Changes of the Nerve-tube connected therewith.* An Academic Thesis.

2. *Om Förlamande Muskelförtvining fortskridande genom Fettförvandling.* Af Oskar Th. Sandahl, Ch. M., Adj. vid, K. Carol. Med. Chir. Inst. Stockholm: 1855. P. A. Norstedt och Söner. 8vo, pp. 91.

   *On Progressive Muscular Atrophy with Paralysis depending on Fatty Degeneration.* By Oscar Th. Sandahl, Chir. Mag., Assistant at the Royal Carolinean Medico-Chirurgical Institute.

3. *Anatomiska iakttagelser.* Af Prof. A. Retzius. Föredrag vid Skand. Naturf. mötet. 1856. (Aftryck utur *Hygiea.*) 8vo, pp. 8.

   *Anatomical Observations.* By Professor A. Retzius. Read before the Meeting of Scandinavian Naturalists in 1856. (Reprint from the *Hygiea.*)

4. *Iakttagelser vid Sjukvården å Upsala stads provisoriska sjukhus under* 1857 *års choleraepidemi.* Af Ph. Aberg. Upsala: C. A. Leffler. 1858. 8vo, pp. 34, with Appendices.

   *Observations on the Sick in the Provisional Hospital at Upsal during the epidemic of Cholera in* 1857.

5. *Om Läkekonsten i Egypten.* Af Kir. Mag. Oskar Th. Sandahl. (Reprint from the *Hygiea.*) pp. 18.

   *On the Medical Art in Egypt.* By Oscar Th. Sandahl, Chir. Mag.

1. It is well known that regeneration of the nerves takes place after they have been divided or excised, but this process of restoration has not as yet been followed in all its histological details.

To investigate the latter was the object of Dr. Hjelt's researches. In his experiments he was in the habit of excising a portion of nerve of four lines in length; the greatest distance at which he saw regeneration take place was 12 lines. The author's observations agree with those of Günther, Schön, and Bruch, in indicating eight weeks as the shortest period in which an excised portion of nerve is so far replaced as to admit of the conduction of nervous influence, though Schiff saw sensation restored in 11, aud Paget in 10 or 12 days. In Schiff's case a piece of an inch in length had been cut out of the lingual nerve of a cat; in Paget's the median nerve had been divided in children of 11 and 13 years. The power of sensation appears to return before that of motion.

The phenomena which ensue on the division of a nerve are of two kinds: some being, that is to say, of a subordinate nature, while others are most closely connected with the violent disturbance of the continuity of the nervous tube, and with the interruption of its connexion with the central parts. To the former class belongs retraction. But the phenomena produced on the section of nerves by irritative changes in the tissue entering into their composition are incomparably more important. The characteristic swelling of the extremity of an amputated nerve depends upon a commencing process of new formation, and consists, in its early period, of an excessive development of nuclei, which lays the foundation for the subsequent evolution of the nerve-tubes. The process of regeneration in nerves is originally a formation of connective tissue, proceeding from the nuclei of the neurilemma, which play therein the most important part. "The nuclei entering into the composition of the nerve-tube seem to give origin to the nerve-medulla, although we cannot more accurately explain the mode of its formation. That a chemical and morphological change takes place in the nerve is beyond all doubt, as the nuclei during this development disappear. Schiff certainly assumes that they persist, and are only covered by the nerve-medulla, but of this I have not been able to convince myself; I must, on the contrary, maintain that while they yield the material for the continued development of the nerve-tubes, they at the same time cease to exist. I allow them only a transient, though an important signification."

Schiff thinks he has observed that the vaso-nerves are regenerated more rapidly than the sensory, and these again than the motor-nerves.

The author concludes with some observations on the anatomical changes in the interstitial connective tissue of the nerves in a state of irritation. It is this tissue, he observes, which

under such circumstances plays the most important part, as the changes of the properly nervous elements must be considered as secondary. His work is illustrated with four well-executed plates.

2. The second work on our list is a comprehensive essay by Hr. Sandahl, on the singular and interesting affection, "Atrophia muscularis paralysans, adiposa, progressiva." We can refer briefly only to the author's observations on the statistics, the etiology, and the treatment of the disease.

Of 75 patients labouring under this affection, 65 belonged to the male, and only ten to the female sex. Of 67 patients, whose ages are stated, 22 were under 20 years, 14 were between 20 and 35 years, 22 between 35 and 55 years, 7 between 55 and 60, and two were over 60 years of age. The disease would therefore seem to spare no age (16 patients were under 15 years), but the largest number of cases occurs during those years when the human frame is ordinarily in fullest vigour.

With respect to mode of life, the patients may be divided into two classes: those whose labour is principally corporeal, and those whose occupation is chiefly mental. Of 73, where the occupation is mentioned, 35 belong to the former, and 30 to the latter class. Of these 38, however, many have been casually subjected for a longer or shorter period to bodily exertion, usually chiefly with the part of the body in which the disease first manifested itself.

As to the etiology of the disease, in nine cases it appeared to be hereditary. In these patients the affection manifested itself during childhood, and always occurred first in the lower extremities. This undoubtedly depends on the fact that these limbs are the first which children exert to any great degree, and is evidently connected with the circumstance that constant or frequently recurring over-exertion of some muscular group seems, in the majority (41) of the cases alluded to by the author, where any exciting cause can be assigned, to have been the proximate cause of the outbreak of the disease.

In 11 cases the disease appeared after cold, and was preceded by rheumatic pains.

Four times it was a consequence of severe pyrexia, three times it appeared to have been produced by onanism, and three times by external violence.

In the remaining cases no exciting cause can be definitely assigned, but the disease set in and was developed gradually and imperceptibly, and, as it would seem, independently of accidental causes. Lastly, it may be added, that in the causation

of this, as of most other degenerative diseases, poverty and misery, as well as anxiety and mental suffering, play an important part.

With respect to treatment, the only means which has produced any improvement, and in some reported cases complete recovery, is local electrization of the affected parts. Fifteen of the cases spoken of were more or less improved under this mode of treatment. But in order that this remedy may be of use, it must be employed in time, while the wasting is still in the incipient stage. If this has advanced further, the employment of electricity is also futile. The paralyzing consumption of the muscles then extends with steadily progressive fatty change, and defies every effort of art.

3. The next pamphlet before us is a reprint from the Hygiea of a continuation of the valuable "Anatomical Observations" of the distinguished physiologist, Professor A. Retzius. The subjects treated of on the present occasion are, the occurrence of foramina in several different parts of the bony wall of the cavity of the tympanum in man; the existence of gray convolutions on the under surface of the fornix and splenium in the brain in man and animals; and, thirdly, the conical papillæ situated over Peyer's follicles in the intestinal canal in the cat and many animals. In connexion with the first of these subjects the writer says: "It appears to me evident, that such circumstances ought to be taken into consideration in practical cases, and that they were not properly understood at the time when special instruments were invented for the purpose of artificially opening the outer wall of these (the mastoid) processes. My attention was specially directed to this subject during a visit to Dublin in 1855, by seeing the distinguished oculist and aurist, Mr. Wilde, open the mastoid cells with a scalpel, as a matter of course, in cases of inflammation of their internal lining membrane, for the purpose of giving exit to the morbid inflammatory secretion, and by this means relieve the tension, assuage the severe suffering, and subdue the inflammation. I was glad to see that the operation was easily performed, and that the results were favourable. I have since myself found that in many cases of so-called otalgia, the pain has its principal seat in these cells or sinuses."

4. Dr. Aberg's observations appear to be very carefully drawn up, but as there is fortunately at present no special reason for introducing the subject of cholera, we shall not in this Number enter upon their consideration.

5. In Hr. Sandahl's pamphlet we have a brief but interesting sketch of the state of medicine in Egypt both in ancient and modern times. The author, who writes from personal observation, having about two years ago spent a winter in that country, remarks that the school of medicine established at Abu-Zabel in 1826, and transferred to Cairo in 1849, though so great an advance in many respects, has as yet borne but little fruit with respect to the population at large. "The Arabian or Fellah in Egypt adheres too obstinately to his old customs and prejudices to allow him to have acquired confidence in the modern art of medicine. He very rarely calls in any educated physician—never if the latter is of the same religion with himself. He usually tries to cure himself by abstinence and the abundant use of water, with warm baths in the commencement of his illness. Otherwise sunk in his blind destiny, he says: '*Allah-hu-akbár, Allah Kherim,*' ('God is great, God is merciful'), and thus most frequently leaves his disease to run its own course. The European physicians in Cairo and Alexandria are therefore employed exclusively by the numerous European population in these cities, and only some few of the higher and richer Turks, who during their travels in Europe have imbibed more modern ideas, or who are placed about the Pacha and the throne, in the more serious cases call in any of the professors in the medical school."

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*Report on the Pathology, Therapeutics, and General Aitiology of the Epidemic of Yellow Fever which prevailed at Lisbon, during the latter half of the Year* 1857. Presented to both Houses of Parliament by command of Her Majesty. London. 1859.

The "Blue Book" before us is, what these ponderous tomes frequently are not, a valuable contribution on the subject of its Report. As might have been expected from its learned compiler, it brings before the profession and the public, in a clear and intelligible form, an evidently truthful and comprehensive account of the fatal epidemic of yellow fever which attacked the inhabitants of Lisbon in the year 1857, and of which we had such conflicting accounts in the newspaper press of the day. The profession will receive this "Report" as fresh evidence of its author's zeal in the cause of medical investigation; and while they honour him, and feel proud of his philanthropic efforts, the public will surely, also, duly estimate the man whose "mission," as he tells us, "to that city on the occasion

in question was one voluntarily undertaken in the interests of science and humanity."

Dr. Lyons is already well known for his valuable researches as Pathologist-in-chief to the British army in the Crimea, and we have no hesitation in stating that his Report on the Pathology, Therapeutics, and General Etiology of the Yellow Fever of Lisbon is not calculated to diminish his reputation as an accurate observer and sound pathologist.

As within the brief limits of a review it would not be possible to do more than cursorily glance at the various investigations and contributions on which the "Report" is based, we shall confine ourselves to a "notice of the prominent points and most interesting sections; and as Dr. Lyons, in his letter to the President of the General Board of Health, specially refers to several of these, we shall give a summary of a few of his observations, with our own views on the questions at issue. "The very full details," says he, "which I have furnished with regard to the clinical history and morbid anatomy of the disease place the nature of the epidemic beyond doubt; it will be at once recognised as yellow fever of very fatal character." We certainly coincide with the author that the details given in the first and second sections of the first part place the nature of the epidemic beyond controversy, for any one who has ever seen yellow fever, in an epidemic form, in the West Indies, or on the coasts of Mexico, in which the disease may be regarded as indigenous, must acknowledge that a more graphic description of the malady than that of the "Report" could not be given. Yellow fever of a most malignant form the disease in question undoubtedly was. Again:—

"The description I have given of the several marked and different forms under which the malady manifested itself, will, I think, be read with interest; and, perhaps, the details I have entered into on this head, and in the section devoted to pathological anatomy, will be found not devoid of practical importance. They indicate to us how the various conflicting accounts given by different writers of yellow fever epidemics in different times and places may be reconciled, on the supposition that in an epidemic visitation one form of the malady was predominant, while in another outbreak the clinical characters presented were those of a far other type of the disease. . . . . However, in closely analyzing the essential phenomena of the several forms of the disease, we find that they are but varieties derived from a common stock."

The truth of these observations will be fully sustained by any one who has had opportunities of seeing several epidemics of yellow fever; and although the observer will perhaps have

seen the disease in its "sthenic" form most frequently, still he will likely have met with the "algid" and "hemorrhagic" forms also; these were the several types which the malady assumed at Lisbon, and are thus characterized by Dr. Lyons in his Report. There cannot be a question that the difference of opinion expressed by several writers on this subject, since the time of Bancroft, has arisen from their not having seen the disease but under a single aspect; hence, one has described what he has seen, and another what he has witnessed, and their descriptions seem to refer to a totally different complaint, while, in fact, each has written of a disease essentially the same, but exhibiting itself under opposite types to the one and to the other. The reporter next goes on to the origin of the epidemic. In speaking on this point, he says:—

"From the first invasion of the epidemic in Lisbon, a belief became wide-spread and general that the disease had been imported. This opinion, it is necessary to add, was not confined to any class of the community; it was shared alike by persons of all ranks of society, by many amongst the educated as well as by the illiterate, and by many highly respectable members of the medical faculty, as well as by the public at large. This opinion, though so generally diffused, was not, however, universally accepted, for both in the faculty of medicine, and from several non-professional persons of great intelligence, I have heard the most distinct avowals of a belief in the entirely local origin of the epidemic, while certain existing conditions were pointed to as manifestly sufficient to account for its causation."

Now hear what Dr. Lyons says, who, as an impartial investigator, is worthy of all confidence, and who evidently is unwilling to commit himself to the doctrine of non-contagion, although we do not assert that he is a contagionist. He continues thus:—

"After most careful inquiry amongst various official persons, and in all quarters in which reliable evidence could be expected in such matters, I am obliged to state that in no one instance did I obtain such a consistent assemblage of facts, or such an array of well-supported allegations as would, in my mind, warrant the conclusion that the importation thereof was even moderately well founded. On the contrary, so vague, and, in some essential particulars, so conflicting were the allegations as to the time, place, and other circumstances attending the reputed importation of the disease, that I do not believe that the popular opinion so generally held as to the importation of the epidemic admits of being reduced to anything like a uniform or consistent statement in writing."

We must confess that we would have been greatly surprised indeed had there been any "*consistent*" evidence of the direct

importation of the disease, and its consequent spread by contagion, for, had such been the case, it would have been opposed to every established fact with which we are personally acquainted as regards yellow fever; and we are perfectly satisfied that in those instances where importation and spread of an epidemic of yellow fever has been seemingly proved, the disease was either no kind of yellow fever, properly so called, but one identical with our own typhus or typhoid; or that, where the malady was really the yellow fever, as in the Lisbon epidemic, the evidence of its importation and spread by contagion was not based upon impartial and consistent statements, but upon some foregone conclusion, popular impression, and loose data, which, upon a closer and more rigid examination, would have been found irreconcilable with facts. Of the force of this statement any one may be convinced who will take the trouble to examine the reports and evidence regarding the two epidemics of yellow fever which appeared in Gibraltar, during the earliest of which, several cities on the coast of Spain, Cadiz, Malaga, and Barcelona, were attacked by this disease also. The idea of contagion of yellow fever, in any form, we imagine, is never now entertained in the West Indies, which may be regarded as the home of that malady. During a lengthened sojourn amidst these islands, and throughout the entire group, we never heard the opinion of its contagious character maintained, and from our personal observation of two very severe epidemics in the Windward and Leeward Islands, and on the "Spanish Main," we can assert that we never saw a single instance in which the disease had not arisen in connexion with some local cause irrespective of contagion, that is, the disease having been communicated from one person to another. Hence, we certainly consider that they who in the Lisbon epidemic believed it to be entirely of local origin, and pointed to "existing conditions as manifestly sufficient to account for its causation," not only took the only rational view of the matter, but were doubtlessly led to adopt the only reliable means for checking its progress and averting its fatality. It is to be regretted, in one sense, that these means were not actively put in force when indications of the coming scourge might have been easily comprehended; "for," says the reporter, "it is proved beyond the possibility of doubt, that true yellow fever, of a malignant and fatal character, prevailed both in Lisbon and Oporto during the year 1856." Then would have been the time to have put sanitary operations in full action; but, as the people of these two cities have never been remarkable for attention to hygiene, we presume they acted

like persons nearer home—they only bestirred themselves when the pestilence struck terror to the hearts of the entire community. If even now the authorities of Lisbon are aroused from the slumber of centuries, and will read in bygone events the lessons of wisdom which dire experience usually teaches, the late calamitous visitation will prove, in the end, a substantial blessing to their beautiful city.

We can well remember the pleasing impressions produced in our mind, many years ago, by the first view we had of Lisbon, rising in grandeur along the slopes of the hills on which it is built, and towering above the stream of the "golden Tagus," which sweeps round its base, and we felt that a city more advantageously situated for drainage and cleanliness, and the other appurtenances of health, could not be found. But our first visit to the shore taught us the folly of being captivated by beauty at a distance, and we soon learned that, with all her advantages of site, a filthier place than Lisbon it would be difficult to discover. At that time, during a lengthened sojourn, we never changed an opinion we had formed, that the wonder only was that Lisbon was ever free from some pestilential disease, so much did it abound in dirt and horrid odours. Scavenging, there seemed to be none; water, an essential for healthy existence, was scarcely to be seen, except in a few public fountains at which the carrier filled his rundlets to hawk the precious commodity through the town. The torrents of rain which usually fall in the autumn were as a special gift of Providence, and swept from the surface of the streets the mass of filth which the indolence of man had for months permitted to accumulate.

We find, from the Report before us, that the lapse of twenty years had not materially altered the internal aspect of Lisbon, and that its inhabitants had paid another heavy penalty for their disregard of former warnings, and of sanitary neglect.

"All parts of the city," continues the Report, "largely attacked by the epidemic present, in common, certain conditions of insalubrity, which may be classed as follows:—Defective water supply, total absence of, or, more commonly, extremely deficient sewerage, total absence or incompleteness of house-drains, privies, and a consequent unclean state of the streets; badly constructed dwellings, with deficiency of light and air, and a want of thorough ventilation; absence or defective condition of tertiary and secondary sewers: when present, such sewers, as well as those of the largest section, were found, by repeated inspection on the part of the Fourth Commission of the Sanitary Congress, and by my own personal observation on many occasions, to be, in a great part, wholly ineffective, being choked with soil, sand, and rubbish, &c."

Surely, then, it is not matter for surprise that, in such a state of things, fever would prevail to a frightful extent, within a limited time, and that its ravages would prove very fatal; and, of course, we will be prepared for the statement set forth, "that where most palpable causes of insalubrity prevailed, there were the most fatal ravages of the epidemic witnessed." Just as was remarked in the epidemic yellow fever of 1723, at Lisbon, and a repetition of former experience, "*most deaths where most dirt!*" "The total number of persons attacked may be estimated at between 16,000 and 17,000, and the total deaths at about 5500." Thus, it will be seen that one in three of those attacked perished—a frightful mortality, when compared with even the very worst of our own fever epidemics.

Having given this brief notice of Dr. Lyons' *resumé* of some points in his Report, we shall proceed to lay before the reader an outline, or rather a slight sketch of the mass of interesting and practical information contained in this valuable Report itself. The first part relates to the pathology and the medical history of the epidemic. This epidemic is shown to have been a true fever, having certain periods or stages, and appearing under several forms. These are classified as the "algid," "sthenic," "hemorrhagic," "purpuric," and "typhus" forms. Of these—

"The algid form was, generally speaking, that which presented the most rapid course, the earliest and greatest amount of prostration of the vital powers, and which likewise offered, in most frequent combination, the most appalling characteristics of the epidemic reunited in individual cases. This form of the disease was presented in a very considerable number of the cases, occurring, perhaps, most frequently in persons of the very lowest classes. It was observed to occur in the very old as well as in the very young, in the lean and haggard, and likewise in those of comparatively robust and well-nourished frames, and in the middle periods of life. It was not limited to either sex. . . . . This class of cases presented extreme and, in general, early and marked prostration of strength, &c."

The Report then goes on to give a most graphic description of the disease, as seen in the algid condition, and the reader is at once reminded of the close resemblance which, in many of its characteristics, it bore to the worst form of cholera; in fact, we have seen cases of malignant cholera which could have been accurately described almost in the very words used by Dr. Lyons in his vivid delineation of the algid form of yellow fever. Here, as in cholera, it is probable that the intensity or concentration of the poison at once paralyzed the animal functions of the nervous centres, and the patient never rallied from the de-

pressing influence of the morbific agent. The circulation suddenly partook of the general prostration, and the secretions were all suspended; hence, probably, occurred what was almost universally observed in the algid form, that the yellowness of the skin, usually so marked a feature in the sthenic form, was absent. Hemorrhages, especially from the stomach—the black vomit—were constantly observed in this form; in fact, from all of the mucous surfaces there was more or less loss of blood.

Of the sthenic form the Report states—

"In marked contrast to the algid were the cases to which we have given the name of sthenic. This class of cases was neither so numerous nor, in some respects, so formidable as that variety just considered, nor, again, as that which we have denominated the hemorrhagic form. I have seen the class of cases in the young, as in boys and girls of from six to ten or fifteen years of age; but it was especially well marked in both sexes at the prime of life, and in those with well-developed muscular frames."

In those cases the fever ran high, the calor mordax was observed at an early date, and the yellow colour of the skin was well marked; irritability of stomach was frequently present, and was often accompanied by black vomit. Hemorrhages, though occasionally occurring in the sthenic form, were not, as may be supposed, nearly so frequent nor so great as in the algid condition, and, of course, the sthenic was the least fatal form, and was that most amenable to the therapeutic measures employed.

"Next to the algid form," continues the Report, "the cases to which we have given the designation of 'hemorrhagic, *par excellence*,' were those which presented the most characteristic, appalling, and impressive features of the epidemic."

In these cases there was marked tendency to almost general hemorrhage, and, after death, "a universal engorgement of all internal parts and organs existed, in combination with more or less profuse parenchymatous and superficial hemorrhagic effusion." Here "Disease, in one of its most appalling forms, held its sway throughout, and Art stood helplessly by."

The "purpuric" form seems to have resembled the algid in its phenomena, only that the former was accompanied by marked pyrexia. It bore, also, resemblance to the purpura hemorrhagica of Ireland, when seen in an epidemic form.

The "typhous" form was not, by any means, common in the Lisbon epidemic, but it was seen occasionally as a primary and also as a secondary type; but, even then, it could not "admit of being, in any way, reduced to the type of the typhus icteroides of authors."

We must now quote a word or two relative to the treatment of the formidable disease under review; and here we must honestly confess that, in our mind, that adopted does not redound much to the credit of medicine, as it seems neither to have been effective nor scientific. Of course, the reporter had nothing to do with that—he merely narrates what he saw.

"The treatment," says he, "of the several forms of the fever which I have seen pursued resolved itself into the use of stimulants, counter-irritants, purgatives, and the employment of special remedies, such as quinia, bark, iron, &c.; blisters to the nucha, epigastrium, insides of the thighs, calves of the legs, and dorsum of the feet, were constantly employed, &c., &c."

Did it never occur to the Lisbon doctors to employ the warm bath, cold or tepid affusion, the first to restore warmth in the algid form, and either, to relieve the dry, hot skin and headach, especially one or other in the sthenic form? Likely, as these gentlemen had not been habituated to the free application of the cleansing element, they had not heard of its use, or they had no faith in that, the efficacy of which they had not personally experienced! Calomel, or any form of mercury, does not appear to have entered into the treatment, and yet, according to our experience of yellow fever in the West Indies, it is the only drug that seems to exercise a marked influence in the relief of the urgent symptoms of that disease; and this is the experience of nearly every one who has practised within the tropics. Many, as we did, gave the calomel in combination with quinia, with the best results; and, unless the Lisbon practitioners were completely ignorant of the treatment of yellow fever as pursued in the West Indies, it is very strange that they did not give calomel either alone, or in combination with quinia,—which they seem to have used in enormous, if not poisonous doses,—a fair trial. The very means that we have found most efficient in enabling the system to bear up against and eliminate the poison, and to restore the arrested functions and secretions, were apparently ignored by the Portuguese doctors. It is certainly true that, in a large proportion of the cases occurring in the algid and hemorrhagic forms, all treatment would have failed, as in these cases the morbific poison had probably altered the vital current, either by acting directly upon the blood, or through the nervous system, and that to a fatal extent: still we cannot but regard the absence of the warm bath, tepid and cold affusion, or sponging, as the rejection of a most powerful therapeutic agent, and the non-administration of mercury, calomel especially, as the casting aside of the remedy which has been

found so efficient in the treatment of yellow fever within the tropics, and of analogous fevers in other climes.

We have now briefly to refer to the post-mortem appearances. This part of the Report is very full and instructive, and the results are most minutely given. To render even a summary of them would be a matter of impossibility within the limits of a review, and we shall, therefore, merely direct attention to the state of the liver, as observed by Dr. Lyons in his numerous examinations after death. Of this viscus he says:—

"The most remarkable, the most constant, and, to my mind—I will frankly avow it—the most inexplicable condition presented in the post-mortem examinations of fatal yellow fever cases, was the state of the liver. I believe it may be affirmed that *some* departure from the normal state of this organ was an absolutely constant condition in all the cases which proved fatal."

And, we believe he might have added, of those that recovered also; for we have ever regarded yellow fever as a disease in which the liver is specially implicated, and, of course, were quite prepared for the statements recorded by the learned reporter. The change observed by Dr. Lyons in the liver, and the results of his tests and examination, which are new to us, led him to believe—

"It was sufficiently obvious, from these investigations, that the marked character of the hepatic lesion was that of fatty degeneration, with accumulation of fatty elements in the otherwise normal hepatic cells."

We must now bring this lengthened notice to a close, as our limits do not permit us to do more than mention that the remainder of the "Report" is taken up with the physical history of Lisbon, including special topography, water supply, state of sewerage, and state of the population as regards food, &c.; then the general and special climatology of Lisbon is noticed, with reference to minute meteorological tables and necrological records; the whole followed by a copious appendix, with meteorological charts and physical maps of Lisbon, showing the parts of the city invaded by the yellow fever; its various elevations, and the districts described in the "Report" as insalubrious.

In taking leave of this excellent "Report," we beg to express to Dr. Lyons our admiration of the zeal which led him to undertake the great labour, and of the energy and perseverance which enabled him to complete the task. We thank him for

this valuable contribution to medical science; and to the members of our profession who take an interest in the pathology and etiology of yellow fever, we can recommend this "Report" as one well worthy of their earnest attention.

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*Von dem Mangel, der Verkümmerung und Verdoplung der Gebärmutter, von der Nachempfängniss und der Ueberwanderung des Eies.* Von ADOLF KUSSMAUL, Prof. D. Med. in Heidelberg. Würzburg. 1859. 8vo, pp. 384.

*On the Deficiency, Arrest of Development, and Duplication of the Uterus, of subsequent Conception, and of the Transmission of the Ovum.* By DR. KUSSMAUL.

THIS is a work peculiarly German, exhibiting all the painstaking research and elaborate detail so characteristic of the writers of that country. The subject is one of but little practical bearing, the cases are infrequent, yet the volume undoubtedly fills up a niche in uterine history.

The introduction is occupied with a detailed account of the development of the human uterus, with an account of the views of Rathke, Serres, Bischoff, Wolff, Gaertner, &c., and then the author proceeds to the general consideration of defective formations in the human subject, double uterus, atresia uteri, obliquity, &c., with cases in illustration. The really important point for us in such cases is the diagnosis, which is confessedly difficult. Dr. Kussmaul states, indeed, that we possess no certain sign of the absence of the uterus in a living woman. Perhaps not, and yet we think that it is possible to arrive at a sufficiently certain practical conclusion. If a menstrual molimen occur occasionally and regularly; if sexual developments have taken place, with the absence of menstruation and a closed state of the vagina at some portion of the canal, there will be grounds of suspicion, but of nothing more, for these phenomena have all been noticed where occlusion has taken place after difficult labour. But if, in such a case, we proceed to examine, with a finger in the rectum and a catheter in the bladder, we may either discover a substance between the two, or such an absence of any body like the uterus as will, in most cases, enable us to come to a conclusion correct enough for practical purposes. Many such cases have been collected by the author, who has added others to show the difficulty that has been felt in coming to a satisfactory decision even after death. In some cases, such

as those by Renauldin, Alby, Meyer, &c., the uterus may be present, but in a rudimentary condition, which would preclude a correct diagnosis. In a somewhat similar case Kiwisch conceived that the uterus was entirely wanting, and other cases of the kind are recorded: so that, upon inquiry, the author expects us to agree with him, that many of the older cases, especially of entire absence of the uterus, are so questionable that we can by no means feel certain about them.

Next, we have a chapter of much curious information concerning the various modifications of rudimentary uterus, according to the part in which the arrest of development has taken place, and the period at which the growth was arrested, either from the fourth to the fifth month, or in the later months of intra-uterine growth. Thus we may have inequality of the two halves longitudinally, a uterus divided into two cavities, a bicorned uterus, &c., &c.; and if it be difficult to pronounce upon the absence of the uterus, it may be equally so in those cases, if the uterus be very rudimentary. The essential ground must be the information afforded by a tactile examination of the abdomen, by the rectum, vagina, and bladder. According to the author, the vagina may be absent, or a shorter or longer blind sac, with no prominence of the cervix uteri at the upper part; or, at the termination of this canal, we may find the lower segment of the womb incompletely developed; or, lastly, the vagina may be much longer than ordinary, and neither the finger nor the sound able to detect the vaginal portion of the uterus nor the os uteri.

Passing on to the consideration of the double uterus, and the bicorned uterus, we enter upon a wider field of interest. In the former cases impregnation was impossible; in these latter cases it has occurred, and it has been adduced as one mode of explaining certain anomalies of generation. Dr. Kussmaul has collected and given in detail all (we believe) the cases of this kind on record, and then follows a very interesting chapter upon the influence of the bicorned uterus (one cornu being incomplete, or both developed) upon the sexual aptitudes of the female. Menstruation is as regular, conception not less facile—pregnancy sometimes occurring in both cornua, or even twin pregnancy in one. There is no peculiar liability to abortion, but rupture of the impregnated, blighted cornu may occur. When one cornu contains an ovum, the other is lined by decidua. The labour and recovery do not appear to be attended with more than ordinary danger, The diagnosis is extremely difficult, if not impossible.

In support of these propositions, the author cites a great number of cases, and, indeed, too much praise cannot be given to him for his industry and care.

We must refer our readers to the work itself for the details, and shall content ourselves with noticing two points connected with double uterus, when both halves are well developed.

What is the influence of double uterus upon labour? In the different forms of double uterus the labour may be natural and easy, although in the greater number on record it has been irregular, and of these many instances are quoted. Among the mechanical conditions which have acted unfavourably may be enumerated too long vagina; the impediment to complete action, from the divided uterus, and the want of consentaneity of action; the discordance between the axis of the uterus and the pelvis; the peculiar position of the os uteri, and the situation of the placenta.

As to the results of childbed in these cases, we find that the larger number have died, and in those cases which escaped, the recovery was irregular.

Very minute and curious are the details of the sections of which we have given so brief a sketch. No assertion is advanced without support; in fact, we should rather say that the statements are logical deductions from the cases collected. It is a volume of importance for the library, and will be highly considered; but we cannot help hoping to see a work for daily use from the pen of an author who has shown himself so laborious, and careful, and accurate. We must not omit to state that the volume is liberally illustrated with plates of the different malformations.

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*A Manual of Medical Diagnosis; being an Analysis of the Signs and Symptoms of Disease.* By A. W. BARCLAY, M.D., Fellow of the Royal College of Physicians; Assistant Physician to St. George's Hospital. Second Edition. London: Churchill. 1859. Fcap. 8vo. pp. 616.

WE are informed by historians that in ancient Rome the priestesses who served at the sacred shrines of Vesta were obliged to divide their period of service into decades; the first ten years being occupied in learning their duties; the second, in practising them; and the third, in communicating the fruits of their experience to others. Thus no vestal was allowed to teach these sacred mysteries until she had qualified herself by long and patient study during twenty years. In our days the period

of literary incubation is considerably abbreviated, and many authors would consider themselves badly used if they should be compelled to withhold their literary progeny until the vicesimal anniversary of their initiation had come round. It may, however, be admitted in their defence that the traditionary mode of instruction peculiar to former ages has been replaced by other and quicker methods of education. This is effected by reducing the respective arts to rules and systems, and by that means making sciences of them.

Without denying the fact that sound sense, quick observation, and constant practice, may enable some to reason justly in their professions without the aid of rules, still, little real advancement can be expected without them. When we read of such a man as Sir Isaac Newton regretting that he had not studied the rigid demonstrations of Euclid in early life, we are furnished with a strong argument in favour of systematic instruction. "Practice joined with rules," as has been well said by Reid, "carries a man on in his art farther and more quickly than practice without rules." When we hear a man boasting that he does not care for rules or science, that he seeks the appellation of a practical working man, in nine instances out of ten we lay it down in our minds that he is an empty pretender.

What is true of other arts is equally true of the art of medicine. Laws and rules are laid down by experienced teachers which throw light upon the obscurities of practice; which enable the learner to acquire a knowledge of the art with more readiness, and to wield its powers with more confidence. The medical practitioner appeals to rules to correct his own mistakes, and to detect those of others, to confirm his judgment, to justify what is right, and to condemn what is wrong.

We are, therefore, always glad to receive any publication which assumes for its object the systematic improvement of medicine as a science. A work of this nature is now before us for review. The rules of Medical Diagnosis are set forth in it by Dr. Barclay as aids to guide us in our analysis of the difficult and variable phases of disease. His book has been already before the public, and the circumstance of its reaching a second edition in little more than twelve months from the date of the first is a sufficient proof that a treatise of this kind was wanted. Without suffering this favourable reception to bias us in our judgment, we shall proceed to examine its contents.

The author has introduced his account of the signs of special diseases with some judicious remarks about diagnosis. "The province of medical diagnosis is to apply the symptoms of disease in general to any particular case, so as to arrive at a

just conclusion as to its true nature and pathology." This is the object of it, and he proceeds to distinguish two grades of attainment of this difficult art; one, the higher form, enabling us to discriminate the finer shades of difference which distinguish forms of allied diseases; the second, of less elevated aim, directed to enlighten the student how to recognise at the bedside the features of the disease, which already he has seen sketched in the lecture-room. Dr. Barclay points out that this, which at first sight may seem an easy task, is not at all such. Diseases are so variable in their aspects at times, that they cease to be readily detected. Family and individual peculiarities so change the appearance of maladies, that it may be laid down that no case is an exact copy of the picture delineated in systematic treatises. Hence, in such works the outline only can be sketched in general characters; it is left to clinical observation to fill in the detail.

We agree with the author in his observations upon the class of symptoms, so-called "pathognomonic," as being those which, of all others, are most likely to lead the mind astray to erroneous conclusions. More especially, he warns us against the very prevalent delusion of expecting to find in the valuable signs of diseases of the chest, afforded us by auscultation and percussion, such decisive indications. Indeed, it is only novices in stethoscopic lore who look for such leading signs; the great masters of the art emphatically assert that no single symptom can be universally taken by itself to represent the disease, but that its main value depends on its association with others. Pectoriloquy was once considered diagnostic of a vomica, owing to the authority of Laennec, but after a time "it fell from its high estate," as pathognomonic of pulmonary caverns—one high authority asserting that he considers it "as the least important and most fallacious of all the physical signs of phthisis;"[a] another, Professor Skoda, declaring that no real difference exists between it and bronchophony. A sign, to be truly pathognomonic of a disease, should stand to it in the simple relation of effect to cause, that is, should invariably and unconditionally, as a consequent, follow the disease as an antecedent. Very few signs will stand this ordeal. The author examines the claims of crepitation to be regarded as pathognomonic of pneumonia, and thus disallows them.

"It is quite true," he says, "that clinical observation has shown in a vast number of cases, that when after death fibrine is found effused into the parenchyma, such an obstruction to the admission of

[a] Stokes on Diseases of the Lungs, p. 406.

air at one period exists, that it enters the lung with a puff of crackling noise called crepitation; but until it can be shown that the noise stands to the fibrine in the relation of effect to cause, it is a false induction to assume that it is a certain evidence of its presence. And when we consider how possible it must be that some other cause of obstruction may produce the same effect, or one so nearly alike that it cannot be distinguished from it, how possible that some other sound altogether may be mistaken for it; when we further know that both these events do continually happen in practice, and that cases of pneumonia do frequently present themselves, in which crepitation is not heard at all,—it will at once be conceded that, though an important auxiliary, it is not an essential point in diagnosis."

In the same way gangrene of the lungs is stated to possess an unerring indication of its existence in the peculiar characters of the expectoration; but however important these are, can we say that pulmonary gangrene cannot exist without them, or are they always indicative of gangrene? If the persevering search after such leading signs had been successful, medicine could be easily acquired; we should only have to give tables of such crucial instances, which, like finger-posts, would at once put us on the sure track. We consider, therefore, that the author has done right in cautioning the young student especially against an useless search for such signs, and in showing him that accurate diagnosis depends on the nature and the amount of evidence, on the number of the signs and their simplicity.

A work on Medical Diagnosis, to attain its end, should, in our opinion, embrace two great objects. One is to collect information about the signs of disease, so as to form what some writers call the natural history of the disease; the second, and almost the more important for the student, is to show him, by some technical method, how he is to reason from the former. Students are apt to be at a loss as well from want of method as from want of medical information. Dr. Barclay has amply provided against the latter want, by a careful summary of the most remarkable symptoms of each disease; the former, namely, supplying the learner with rules analogous to the art of analysis of logical writers, has received from him less consideration. We do not deny that there are some excellent instructions given by him on this head, but in our mind they do not meet the difficulty. He may consider that this is a matter for the learner rather than the teacher, and say with Dr. Johnson, "I have given you reasons, sir; I am not bound to give you understanding." Still knowing, as any clinical instructor knows, the utter confusion of purpose which the student manifests at first,

and the slow and laborious process by which he corrects his mistakes, we would gladly hail any easy means by which the student can apply the knowledge which he has acquired to the case before him.

Any such method, to be successful, must be founded, as the author justly observes, on careful induction. The materials for such induction are found in clinical observation and pathological research, and the point at issue is, how to attach each separate group of symptoms to its appropriate chain of morbid lesions. But how are we to judge that this is done correctly? We must depend, says the author, on the amount of evidence in the case, on the proper value of each symptom, and, finally, on verifying the result by comparing it with the known phenomena of the disease. But, it may still be objected, all these points can only be ascertained from long experience, and how is the unpractised student to set about applying his scanty amount of knowledge to the case before him? Herein lies the great difficulty of clinical teaching. The student is expected to possess no small share of acquaintance with the principles of medicine before he can make out a particular example. The author compares the investigation of a case to the properties of certain figures in geometry, which we arrive at by a series of eliminations. For instance, let us draw four lines to as many points, so as to inclose a quadrangular space; we thus obtain a quadrilateral figure in its most unlimited signification. We next eliminate the irregular direction of two opposite sides, making them parallel, and reduce the figures to trapezoids. We proceed to make the two other sides parallel, and are confined to parallelograms. Still further, we reduce these parallelograms to rectangles, by making all the angles right angles. Lastly, by making the diagonals equal, we arrive at our final conclusion—a single result—the square being the only quadrilateral figure which meets such requirements.

Such is the illustration presented to us by Dr. Barclay of our mode of reasoning in medical diagnosis, and if medical phenomena could be as certainly depended upon as mathematical, if they had the same precision of application, and could be expressed in fixed and invariable formulæ like the latter, we may adopt the above process as an example of the course of reasoning which takes place in the mind when investigating a particular case. Unfortunately, such precision cannot be obtained in medicine, and especially for two reasons, which have been put very clearly by John Stuart Mill—namely, the plurality of operating causes, and the intermixture of effects. In medical inquiries we find few phenomena which

may not arise from more causes than one, and equally we find that sundry effects may flow from a common cause. Hence, when we seek to apply these compound causes or effects, as if they were uncompounded, we are brought suddenly to a stand-still. In mechanics, the composition, or division of forces, can be calculated to a nicety; in chemistry, the action of two bodies may totally neutralize each other, and produce a new compound different from the components, but with sufficiently distinct characters of its own; but in medicine, if two organs be simultaneously attacked, the phenomena are not the sum or difference of each single set of symptoms as in mechanics, or a compound altogether different, as in chemistry, but two sets of phenomena blending each with the other, and reacting in various ways. For example, suppose that we possess sufficiently clear ideas of pneumonia as an inflammatory affection of the lungs—an epidemic of typhus fever occurs, accompanied by a sudden determination to those organs, and so modifies the symptoms that they cease to be the same certain exponents as before. The whole subject must be again reviewed; our former conclusions modified, and a new chapter introduced into the history of this disease.

In admitting induction as the true basis of medical diagnosis, our best guide is the great founder of the inductive method himself. In his "Novum Organum," Bacon lays before us the whole process, and there is little doubt but that it is the mode of reasoning which goes on in our minds in forming our opinions without our being aware of the separate steps. Bacon lays it down, that our purpose is to interrogate nature in a regular, methodical manner, and not to jump to a conclusion by hasty anticipations; he, therefore, proceeds as follows:—First, obtain as accurate and comprehensive a knowledge of the subject as you can. Having so done, select some given phenomena, and make a table of as many cases as you can collect, which contain the phenomena in question. Secondly, make another table of all the cases which resemble the former, but which want the given phenomena. Thirdly, compare those cases together which contain the phenomena *secundum magis et minus*, that is in a greater or less degree. Fourthly, reject or exclude such cases as do not possess the common conditions. Finally, in Bacon's quaint language, gather your vintage of such fruits as remain.

Let us take an example to illustrate Bacon's meaning. A person is before us who complains of pain in the right inframammary region; we percuss the part, and find considerable dulness. This is the given phenomenon, from which, as known to us, we are to argue by induction to the cause, which is un-

known. We first proceed to tabulate all the instances known to us, which may be attended with dulness on percussion; for instance, empyema, pneumonia, phthisis, œdema, hepatic congestion, and so forth. These are the affirmative instances of Bacon, or those which possess the given nature. Secondly, we form another table of diseases of these same organs, which exhibit allied symptoms, but want this particular sign; such, for instance, as emphysema, bronchitis, &c. These are the negative instances, and should always be appended to the former. Thirdly, we form another table of those cases which present the given sign in a greater or less degree. Thus, dulness on percussion occurs in phthisis, but it is less complete; dulness occurs in empyema, but it is extreme, &c. We next proceed to eliminate. The dulness cannot be from phthisis, as this disease usually attacks the upper, and seldom the lower part of the lung. It is not from empyema, because the existing sound is not so totally dull as in that ailment, and varies little with different postures, and so on, enabling us, finally, to conclude that the cause of the dulness is consolidation of the lung from inflammation.

We have offered the above merely as a rough example of the utility of Bacon's method as applied to medical diagnosis, and we believe that this process goes on with most minds, though often unconsciously, and not in regular order. But it is the regularity of the process that constitutes its whole merit. The induction is not complete if we omit a step, and no man can be an accurate diagnostician who does not follow some similar course to what Bacon recommended. The force of habit enables an observer to do this rapidly, and some highly gifted minds can pass to the conclusion, as it were, *per saltum*, but though the intermediate steps excite less notice, the result is the same.

A humorous illustration of the analysis of the inductive method is given by Lord Macaulay, to which we may be excused for alluding as bearing incidentally on the matter in question: "A plain man finds his stomach out of order; he never heard Lord Bacon's name; but he proceeds in the strictest conformity with the rules laid down in the second book of the 'Novum Organum,' and satisfies himself that minced pies have done the mischief—'I ate minced pies on Monday and Wednesday, and I was kept awake by indigestion all night.' This is the *comparentia ad intellectum instantiarum convenientium*. 'I did not eat any on Tuesday and Friday, and I was quite well.' This is the *comparentia instantiarum in proximo quæ naturâ datâ privantur*. 'I ate very sparingly of them on Sunday, and was very slightly indisposed in the evening; but

on Christmas Day I almost dined on them, and was so ill that I was in great danger' This is the *comparentia instantiarum secundum magis et minus.* 'It cannot have been the brandy which I took with them, for I have drunk brandy daily for years without being the worse for it.' This is the *rejectio naturarum.* Our invalid then proceeds to what is termed by Bacon the *vindemiatio*, and pronounces that minced pies do not agree with him"[a].

The author classifies symptoms into the *subjective* and the *objective*—two phrases borrowed from the Germans, which puzzle students (and even wiser heads) a good deal. It might, perhaps, have been better not to have introduced them into medical nomenclature, as they necessarily intermix much with each other. They have been borrowed from metaphysics, in which their meaning, though bearing a stricter application than in medicine, is not without confusion. The most simple application of this phraseology seems to be, that a subjective phenomenon occurs in the subject himself, while an objective is one which occurs outside him, that is, in the external world. "Our certitude of the *object* is that of a thing to be perceived; our knowledge of the *subject* is that of the mind perceiving it"[b]. The medical use of the words is not the same as this. According to Dr. Barclay, "*objective* phenomena are those changes in the condition of the vital functions of which the observer becomes conscious by his own perceptions; while *subjective* have special reference to the sensations of the patient." This interpretation seems intelligible enough, but the question is, whether this is the accepted meaning of the words. In the metaphysical sense, both the above forms are, strictly speaking, subjective, as they occur in the subject—the difference being, that in the first case they are appreciable by outward observation, in the latter by the person himself. Among writers who use these expressions we find Dr. Carpenter, whose acquaintance with foreign medical literature gives weight to his opinion. In his Human Physiology he distinguishes "*subjective sensations* as those arising from internal causes, from *objective sensations*, which result from a real material (that is, external) object." We confess, therefore, that we have an objection to the use of words of this class, whose definitions are not yet fixed by sufficient authority, as they convey to the student rather misty ideas. The old terms, namely, symptoms and physical signs, may, we think, be conveniently substituted for the more high-sounding epithets of subjective and objective phenomena.

[a] Macaulay's Life of Bacon, Essays, vol. i., p. 404. [b] Mansel's Logic, p. 115.

We turned with some curiosity to the chapter on fevers, to see the author's opinions on that much litigated question, viz., the identity or diversity of typhus and typhoid fever. He gives no decided opinion on the disputed point, but seems to incline to the hypothesis of a single disease, susceptible of various modifications. He gives a fair statement of the doctrine of the French school, namely, that fevers form two distinct diseases, generated by two distinct fever poisons, marked by two distinct groups of symptoms, and terminating, when fatal, in two distinct sets of morbid lesions. As the French pathologists aver that the same cause cannot generate fevers with spots of dissimilar kinds, it is plain that the correct discrimination of fever-spots would, on this theory, lead also to the true diagnosis of species of fevers. He gives, therefore, their descriptions of the forms assumed by fever-spots without endorsing them as distinct species, viz., that petechial or exanthematous typhus usually exhibits a copious eruption of mulberry-coloured petechiæ, little, if at all, raised above the surface, consisting mostly of retarded or extravasated blood, and therefore persistent under the pressure of the finger, and associated with a general duskiness of the skin; while the ileo-typhus, dothinenterite, or typhoid, is represented by a rose-coloured rash, more or less elevated over the surface, less abundant than the former, and disappearing upon pressure, but reappearing when pressure is removed. The accompanying characteristic symptoms of these fever-spots are, according to French authors, equally well defined, the former being marked by great depression of the vital functions, softening of the muscular tissue, both voluntary and involuntary, and great disorder of the sensorium; the latter by diarrhœa, abdominal tension and tenderness, and enlargement, with ulceration of the agminated intestinal glands. Our own opinions upon this matter have been often brought before our readers, and the researches of Law, Graves, Stokes, and other distinguished pathologists, whose careful observation of the Irish fever—too common a visitant, unfortunately, of our country—all tend to show that there is but one fever-poison, that the various aspects of fever types are produced by accidental modifications, and that this is proved by both forms occurring in the same epidemic, and by its exhibiting different appearances in persons in the same house and members of the same family, doubtless from some antecedent circumstances affecting their health at the time.

While upon this subject, we may say, with regret, that we looked in vain for any notice of the auscultatory signs of the typhus affection of the heart-muscle, so incontrovertibly laid

down by Dr. Stokes, and so valuable in its therapeutic indications. The important complications of pneumonia and bronchitis with typhus are but slightly alluded to, and in the brief account which he gives of intermittents we lack the accurate and beautiful description of a paroxysm as given by Cullen. Eruptive fevers also, as constituting a class upon which young practitioners should be well made up—seeing that they have in the maternal public critical and unsparing judges—might advantageously be treated in a more comprehensive form. Indeed, we would recommend our author to revise, in his next edition, the whole subject of fevers, as not being, in our opinion, adequately to their importance, represented in his work.

In looking to the diagnosis of thoracic diseases, we are much gratified to state that it has been well studied and carefully written. The author has devoted a large portion of his space—about a fourth part of the work—to this interesting subject, and he has given an excellent *resumé* of our knowledge upon auscultation and percussion. We had marked some passages respecting cardiac diseases, in which allusion to important symptoms has not been made, but our space allows us to take but a passing glance. For instance, in adhesion of the pericardium the immobility of the heart in various postures is not given, as also the great reduction of the normal number of pulsations which occasionally attends this change. In fatty disease of the heart we miss that important symptom first prominently noticed by Adams, the tendency to frequent losses of consciousness, without being followed by paralysis. In the same way, the indications of patent aortic valves, so clearly laid down by Corrigan, are only partially given; and the diagnosis of cirrhosis of the lungs, first described by that author, and bearing the high sanction of Rokitansky, is totally passed over. We admit, however, that, in so extensive a subject, particular subjects must be contented with a general consideration.

The chapters on the examination of the urine and the diseases of the urinary organs, though not comprehending more than about forty pages, contain a very good account of the matter. The student has before him, in a small compass, the several tests of the abnormal elements most constantly met with in the urinary secretion.

In conclusion, we have great pleasure in saying that, though in our preceding remarks we have differed from Dr. Barclay on some points, yet, regarding his work as a whole, we consider it as well executed. It is plainly the production of a physician who has studied disease for himself at the bedside, and has written down the results of his observations. It contains a

large amount of knowledge on the subject on which it treats, communicated in an intelligible style. The philosophical remarks in his introductory chapter show that he is familiar with the writings of our best logical authors, and we are always glad to see medicine treated in that enlarged and comprehensive manner which shows that it can be best elucidated by men of general education. He has adopted in his work a plan to suit readers of various kinds, which we notice without any comment, namely, giving under the several heads information of a more recondite nature, in a smaller type, so that the less experienced or more indolent reader is not required to depart from the ordinary text. On the whole, we regard it as a great improvement on the valuable little manual of Martinet, and calculated to be of great advantage, not only to the medical student, but to the professional reader. We consider that the public stamp of approval, in calling for a second edition, has been fully sustained.

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*A Manual of Operative Surgery on the Dead Body.* By THOS. SMITH, F. R. C. S. With Illustrations. London: Longman, Green, Longman and Roberts. 1859. 8vo, pp. 139.

IT has been happily observed that the character of the age may in a great measure be inferred from the nature of its literature; and that which is true of the general literature of a country is still more true when applied to any particular profession. Before medicine became a science, whilst surgery was still in the hands of barbers, the character of our medical works was necessarily of a conjectural, superstitious type, couched in language studiously of a mystic and unintelligible nature, for the sole purpose of, in aimless verbiage, concealing the ignorance of those who pretended to instruct their brethren. Considerable weight was attached to things in themselves unimportant, and so set down in the writings of the periods to which we allude. How frequently do we meet with sudden bursts of light, revealing to us great facts; their vast practical value duly appreciated by their enunciators, and, perhaps, by some few other kindred minds, but announced only to meet the fate that almost inevitably awaits the promulgation of any view put forward in advance of the age in which we live—vouchsafed to us, as it would almost appear, to excite our special wonder at the mental blindness that will not allow itself to see, at the intellectual deafness that will not hear, but prefers pursuing its own dark

path, uninstructed by the lessons taught in the most practical, the most valuable of all books—Nature's own.

Reflections such as these have been suggested to us by the title of the book which we propose to bring presently under our readers' notice. If our times are hereafter to be measured by the New Zealander sitting on a broken arch of London Bridge, apart from every other consideration, by the nature of the books that issue from our press, there can be no doubt of it that he will come to the conclusion, that a spirit of an earnest, practical nature was dominant amongst us; that great sacrifices had been made by us at the altar of our common welfare, not the least of which is that which has opened up to our rising generation access to our great public service, not by favouritism, but by public competition, in which the battle is to the strong, the race to the swift—a system which calls for a higher order of mental culture, and which renders such a work as Mr. Smith's not only acceptable to the general mass of students, but an absolute necessity to many amongst them. For, to be successful at these competitive examinations, the candidates must perform on the dead subject, in a creditable manner, the several surgical operations which hereafter he may in the course of his practice have to execute; and, to enable him to do so, his attention must have been turned in this direction, and the assistance of a master, or of some good work on the subject, is also essential.

It has long been a matter of surprise to us that our several surgical corporations had not taken into consideration this question of operative surgery on the dead body long since. Founded for the purpose of guaranteeing to the public that their alumni were to all intents and purposes practical, efficient surgeons, what measures have been hitherto taken (or at all events until a very recent period) practically to test their knowledge in this department of surgical education? At the present moment we believe ourselves to be correct in stating that the only College which does take any steps in this direction is the London one; for we utterly ignore the parrot-like repetition orally of the steps of an operation as being any proof of the candidate's capacity for performing it. In which of the curriculums of our colleges of surgeons is a course of operative surgery insisted upon at the hands of their candidates? Lectures, demonstrations, dissections, to no end, are demanded; but not one word is said of that department by which hereafter character is to be gained or lost; or higher consideration still, life at a moment's notice is to be saved or sacrificed. It is true that it

may be advanced, opportunities are afforded to many of us in our larger towns, previous to any capital operation, of practising it beforehand on the dead subject; but does such a plea as that excuse the laxity of discipline in our surgical colleges, which certifies that the party holding their diploma is, to the fullest extent of the word, a surgeon, without ever having in his preliminary education insisted on his performing *with his own hand* a course of operative surgery; or, at his final examination, indirectly put such an enactment in force, by practically testing his manipulative powers. But how stands such a plea as this with regard to the infinitely more numerous members of our profession, whose lot is cast in our several dispensaries, workhouses, hospitals, naval and military services, &c.? They have no such opportunity at their command, and yet services of an operative nature are expected at their hands; for their education in the performance of which, no adequate provision has as yet been made. So simple a remedy for this crying evil is ready at our hands, that it surely must surprise even the most eminent of the *nil admirari* genus to find that it is not enforced by all of our British Colleges of Surgeons: it is this, to substitute for one of the courses of dissections, anatomical lectures, and demonstrations, at present required of the student, a course of operative surgery on the dead body. The student would materially benefit by such a regulation; the public would reap incalculable benefits from it; no additional expense would be entailed on the student; his educational curriculum would not be extended, whilst the teachers would not suffer any pecuniary loss by this substitution of one course for another. It is our firm opinion that the adoption of some such regulation is but a question of time, and fortunate for all parties will it be when it is *un fait accompli.*

Apologizing to Mr. Smith for this long digression from his work, which, however, has the merit or demerit, as the case may be, of having directed our thoughts into this channel; we must now proceed to the task of expressing to our readers our opinion of the manner in which he has discharged the duty which he has undertaken. Mr. Smith has provided for the wants of our coming generation of surgeons a most valuable book; small indeed, and unpretending in form, yet it contains an amount of information on the subject of which it treats, that books of treble its size do not afford. From its very nature we cannot look into it for novelties or matter hitherto unknown to the operative surgeon; but it does contain clear, well-written, concise, and accurate directions for the performance on the dead subject of all the principal operations, with but some trifling

exceptions, likely to be called for on the living human being; and what we principally admire in his book is, what may at other hands lay him open to the charge of numerous omissions in his description of the different methods by which operations having the same object in view may be performed. For instance, in describing the operation of amputation of the arm at the shoulder-joint, instead of leading the bewildered student through the maze of some five-and-twenty different methods suggested for the performance of the operation, he confines himself to a description of but two—the operation by an upper and lower, and that by an anterior and posterior flap. This, in our opinion, is as it should be: the work is intended as a Manual of Operative Surgery on the dead subject, and not as a complete treatise on this subject with reference to the living; and the introduction of such unnecessary details would but serve to defeat its own object, and mar its utility with the class for whose benefit it has been specially produced.

From these remarks our readers are not to infer, however, that our recent advances in operative surgery have been in any way slighted; far from it; for we have in this respect the work brought down to the latest period. In addition to Chopart's and Hey's (as we love to call it) operations for removal of portions of the foot, we have full directions given as to the method in which Syme's amputation should be performed, with Pirogoff's improvement on it, and Eben Watson's on Pirogoff's. Teale's method of amputating by rectangular flaps has full and ample justice done it. Allarton's and Lloyd's operations for lithotomy are faithfully described; and Butcher's operation for excising the wrist-joint also finds its place here, as indeed it must in every work on the subject that has in its author an admirer of sound conservative surgery, based on an intimate acquaintance with the structures of the parts, and a thorough knowledge of the principles that should direct the surgeon in the practice of his profession.

Having said so much in favour of his work, we trust that Mr. Smith will not take it amiss our offering him a few suggestions for his next edition, that we predict for him, and that at no far distant day: they are offered in the true spirit of professional brotherhood, and not in the least meant to detract from the value of his present production. We would then recommend a more liberal introduction of plates into the body of his book, illustrating the operations which he describes. What we have are in themselves admirable, but they are not sufficient for the wants of the student. We would also suggest, in the form of foot-notes, references to the original sources

from whence he has taken his description of the several operations in his book; this would enable the industrious student more easily to refer to them for fuller particulars, which we admit would be out of place in a work such as his. We would also suggest the introduction of the description of some of the minor operations, such as those for hare-lip, entropium, circumcision, etc., which, though of a less important nature than those he has described, still require for their successful execution a neatness of manipulation, only to be acquired by constant practice. To repeat the steps of these operations, it is by no means necessary for the deformities, for the relief of which they are performed, to have an absolute existence; nor would their execution at all interfere with the subsequent more important operations on the same subject. One other operation also, in our opinion, should be enforced on the pupil's repeated attention, and on every practicable occasion he should be urged to perform it —that of passing the catheter on the dead subject; by this means alone will he qualify himself to perform on the living subject what but too frequently he will find to be the most delicate and the most trying one in the entire exercise of his vocation.

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*Preventive Medicine.—Statistics of Small-pox and Vaccination in the United Kingdom, and the necessity for a better system of Vaccination in Ireland.* Read at the Meeting of the British Association (Section F), Aberdeen. By William Moore, A.B., M.B. T.C.D., M.R.I.A., &c., Physician to the Hospital for Diseases of Children. Dublin: Fannin & Co. 1859. Pamphlet, pp. 14.

Our first impression, upon observing the words "Preventive Medicine," heading the title of this brochure, was, can it be possible that a question of such magnitude is fully discussed in so small a compass? Upon a closer examination, however, we found that but one topic of the long list of those contained in the category of Preventive Medicine was considered, namely, vaccination; and that only so far as it relates to Great Britain and Ireland, more particularly the latter portion of the United Kingdom.

When the author of a paper deems his production worthy the attention of a section of the British Association, such paper is presumed, by the general reader, to contain, both as regards originality of ideas and novelty of facts, pabulum for the minds of no ordinary individuals. The impress of the British Association on the title of any pamphlet has an imposing effect upon

intellects of common capacity, and, therefore, previous to the perusal of Dr. Moore's pamphlet, we were anticipating extraordinary instruction and peculiar pleasure. We have now to confess that our hopes were blighted; and, moreover, having given to the subject the most careful attention, we are constrained to the conclusion, that the business of the British Association, when last they assembled at Aberdeen, must have been very slack, or, the time—albeit 'twas inconsiderable—necessary for the reading of it would not have been wasted on such platitudes[a]. The object of the pamphlet is to inform us—and did we need "a ghost to tell" it ?—that vaccination is not properly conducted in Great Britain, but more especially in Ireland. For this purpose the following authorities are quoted:—Barclay; Greenhow; Simon; Registrar-General's Report for December, 1858; Farr; Whitehead; Mr. Moreton's Statement in Petition on Vaccine Bill, 1856; Balfour in Medico-Chirurgical Transactions; Seaton; the Registrar-General's (of Scotland) Quarterly Returns for August, 1859; the Dublin Statistical Society's Committee's Report on Registration of Births, &c.; the Report of the Dublin Cow-pock Institution for the year ending March, 1859; the Census Report of Ireland, 1841 to 1851; and the Vaccine Act of August, 1858.

From all these sources copious extracts are taken, or portions paraphrased; and, seeing that the said sources amount to sixteen in number, and that the pamphlet itself is composed of but fourteen small pages, it necessarily follows that not much room remains for original matter.

We have not time to enter into any minute scrutiny of the figures produced from the various sources above enumerated; were we even to find them in error, the fact would not be altered, it being conceded by every practitioner that vaccination is most imperfectly carried out in this part of the United Kingdom. The figures collected by Dr. Moore might, however, legitimately form a very fair preface to subsequent new and striking suggestions for the removal of those obstacles which render vaccination, at present, little better than a farce in this country. But we regret to say that Dr. Moore has only harped upon the old songs so often dinned into the ears of authority—some of them, truly, excellent ideas, but not one of them Dr. Moore's. Thus, firstly, compulsory vaccination; in order to carry this

[a] It is well known to learned and scientific men that several members of the Engineering profession have for some years back availed themselves of the Annual Meetings of this Association as a sort of "advertising van." We regret to find that some of the members of our own profession have endeavoured to turn it to a similar account at the last Meeting at Aberdeen.

out efficiently, secondly, a registration of births, marriages, and deaths Act; thirdly, direct government control, unconnected with the Medical Charities Act: all backed up by the aid of existing authorities, and private individuals of influence, whether, it is presumed, they be clerical or lay. Such are the *novelties* offered by Dr. Moore to the Government and the British Association.

The following questions must naturally arise amongst those unconnected with the profession, and even amongst some of the latter in the sister kingdom, to whom the affairs of this country offer but slight interest:—Can such a state of imperfect vaccination have existed in Ireland so long unnoticed? Ought we not to be much indebted to Dr. Moore, who, spurred by philanthropy, has, for the first time, brought the truth to light before a section of the British Association, and offered a panacea for his country's vaccine defalcations? Is there such an institution in Ireland as a Cow-pock Institution, and yet are its rulers so deaf to the cry of the people to save them from variola, that they have remained so long silent, and thrown the onus upon our author?

Thus it occurred to us; and having made due inquiry, we were favoured with a series of Reports of the Cow-pock Institution, to which we shall presently advert, when it will be found that Dr. Moore has not been guilty of anticipating the governors of that valuable establishment, and that his pamphlet contains nothing novel nor original.

We shall take the liberty of inserting a few extracts from the reports of the Cow-pock Institution of this city: first, to show that those eminent physicians and surgeons who direct it have in nowise been ignorant of the defects in our system of vaccination, or of the duty they considered to be reposed in them of exhibiting those defects to Government and the public; and next, that they also considered carefully the remedies most likely to prove efficacious to secure immunity from small-pox contagion; and, lastly, we shall give an extract of Dr. Moore's views on this point, as they appear in his pamphlet, to show the peculiar coincidence of perfect identity with those of the directors—the date of publication alone differing.

So far back as 1854, the Directors of the Dublin Cow-pock Institution, in noticing the increased demand for vaccine lymph, say—

"The necessity for this increased supply has arisen, in part, from a greater number of poor-law unions subscribing; *but more particularly from the difficulty recently experienced in inducing the parents of those undergoing the process of vaccination to bring*

*their children back to the several dispensaries at the time most suitable for taking from the vesicles the recent lymph for further use, and thus enabling the professional attendants efficiently to carry out a system of active, continuous vaccination, as also to keep up for contingencies a moderate supply.* From the numerous letters received at the Institution, referring to this subject, the inference is, *that a very great many do not return at all; while, of those who do, a considerable number are presented for observation, either with the vesicles injured—the result of accident or negligence—or, as is most frequently the case, after the eighth day, when the contained fluid is no longer adapted to the purposes of healthy vaccination. Numerous communications have been received from country practitioners, complaining of these difficulties, and on these grounds explaining the necessity, thus forced upon them, for their very frequent applications for vaccine virus.*"

In 1855 there is the same story of increase of issue from the Institution, and yet not from increase in the number of subscriptions, but from an impossibility of obtaining fresh vaccine at the dispensaries, in consequence of absence of proper inspection. Thus—

"This demand is, as has been previously stated, in great part referable to the difficulty experienced in inducing parents to bring back their children to the several dispensaries at the times appointed for inspection, and procuring from the successful cases a supply of lymph for further use. Indeed, judging from the numerous letters received from various quarters, it would almost appear that few think of returning unless when the attempt at vaccination has completely failed, and of those who may happen to bring back their children, rarely any show themselves till the period when the process has been so far advanced as not to admit of the contained matter being further applicable for healthy vaccination; a negligence, in some few instances, the result of apathy, but, in the majority, having its source in an unwillingness to allow the vesicle to be opened, lest the so doing might lessen the protective power of the vaccination, or, as it is ignorantly stated, cause the arm to inflame. This popular error is not by any means confined to the poor and uneducated, being frequently met with as influencing the more respectable, and what we ought to expect to be more enlightened classes of society, to remove or do away with which every reasonable effort ought to be made."

In 1856 the same complaint:—

"The explanation of this increased demand is to be found in a wide-spreading unwillingness on the part of the people (and that not merely confined to the poor) to permit, in cases

of successful vaccination, lymph to be taken from the vesicle; and, the more surely to carry out their determination of not having the vaccinated arm interfered with, the parents either do not bring the children back at all, or, if they do return, they are found not to present themselves till the twelfth day, or later—a period when the contained matter is no longer available for inoculation. The consequences to the public of this prejudice and negligence are, firstly, a great uncertainty as regards the success of the operation generally; and secondly, a withholding of the material, so as to render it impossible to keep up a local stock of recent lymph, a desideratum of the utmost importance with respect to the furtherance of successful vaccination."

These three extracts sufficiently demonstrate that the directors were aware of the obstacles in the way of perfect vaccination, from unwillingness, apathy, ignorance, and misapprehension on the part of the people, and also from the absence of general and timely inspections. But there is another terrible hindrance, of which the yearly reports of Cowpock Institutions take due notice. It appears that in 1857 the demand for lymph was on the decline, and yet vaccinating in the country districts was not proportionally increased, so as to lead to the supposition of a more perfect system of inspections. We find in the Report of that year the following extract:—

"Many popular errors contribute to retard the progress of vaccination; amongst others, there is a prevalent opinion with the lower orders, that vaccination distributes the infection of variola, and yet, with strange inconsistency, they prefer inoculation with small-pox to that with cow-pock; a practice certain to spread, far and wide, the former terrible disease, which might become ultimately extinguished if vaccination were generally resorted to, and variolous inoculation totally abolished."

Again, in 1858—

"These numbers, as compared with those of the two preceding years, show, we regret to say, a falling off in the demand for lymph and vaccination.

"It would appear that this double decrease is attributable to carelessness in the people at large as to the protection of their children from small-pox, or to a want of confidence in the efficacy of vaccination.

"It is true that the public are occasionally aroused by the alarm consequent upon the sudden appearance, in certain localities, of a few cases of small-pox, either occurring naturally, or arising from the operations of itinerant small-pox inoculators.

"There is then an immediate rush of applicants to the dis-

pensary in the neighbourhood, and a demand for lymph from the Institution in Dublin; but this continues only for a short time, subsiding as soon as the cause for the alarm has passed away.

"It may be remarked, that a diminution of the number vaccinated last year was observable, not only in the Institution, but was even more evident among the dispensaries throughout the country, referable, in addition to the causes before stated, to a misgiving in the minds of the poor as to the efficacy of vaccine inoculation, carefully inculcated by itinerant small-pox inoculators, to serve their own ends, and to the manifest injury of the public."

Thus it will be seen that so far back as the year 1854 the Governors of the Dublin Cow-pock Institution knew full well the causes of imperfect vaccination in Ireland. We shall now endeavour to show that they were also alive to the necessity of the adoption of some measures to remedy the defects, and that they offered to Government many excellent suggestions.

In 1854, when regretting that some measure was not adopted to secure a return on the eighth day after vaccination, they complain of—

"The withholding a fair supply of recent and consequently more efficacious lymph; and secondly (what in a public point of view is of even greater importance), the entire uncertainty as to the result, in each individual instance. It is quite impossible," they say, "without the due and repeated inspection of the arm, to arrive at anything approaching a satisfactory conclusion on the matter; hence it follows as a consequence of such neglect, that the benefit sought to be conferred upon the public, by affording, apparently, every facility, for the furtherance of vaccination, is rendered comparatively null and void."

To meet this, to a certain extent, they recommend that the Poor Law Commissioners should sanction the deposit system, as adopted at their institutions, and as was formerly in vogue during the old dispensary regime, as follows:—a trifle, no matter of how small an amount, of money, or something in kind, was deposited, to be returned upon the appearance of the person vaccinated, on the eighth day of the vesicles' progress. In the Dublin Institution one shilling is required, which is forfeited if there is no appearance on the eighth day. The Report in 1855 contains the following warning:—

"They (the directors) would earnestly endeavour to impress upon the minds of those in authoriry the importance of the consideration, as to how far it might be practicable to adopt such measures as would be calculated to insure a second, and,

if possible, even a third visit, from the applicants for vaccination; otherwise they are compelled to fear that the whole matter must remain in a state of unprofitable uncertainty, a vague inference as to the result being the only conclusion that can be arrived at."

Again, in 1856, after drawing attention to the great increase of the prejudice in favour of inoculation with small-pox virus, and the connivance of parents with the itinerant small-pox inoculators, for the purpose of evading justice, the directors again urge the deposit system, pending some Government arrangement, which they hint should be put into action; thus:—

"The only remedy for this grievance—fraught with so much danger to the public at large—is to be found in adopting whatever means shall be deemed most effective for the carrying out of a scheme of complete and carefully *superintended* vaccination, whether, as is contemplated, by compulsion, or, as has been previously adopted, by remunerating the medical attendants of dispensaries for their labours in this department.

"Compulsion is, in itself, at all times more or less disagreeable; but when, as in the present case, it happens that the educated and illiterate are found acting in combination, it becomes a subject of grave consideration, if such a mode of proceeding be not imperatively demanded.

"The recent revival of old and obsolete objections affords additional evidence in favour of the necessity for some decided measures being adopted in order to secure the most rigid attention to vaccination, as well on the part of the public as on that of the profession. And here the directors would suggest (pending the adoption of more effectual means) the revival of the deposit system, which they have found to answer so well in connexion with their Institution, and since the suppression of which—as asserted in the communications received from the body of dispensary physicians at large—it has been found utterly impossible to carry on, even an imperfect vaccination throughout the country.

"*In conclusion, and in support of their expressed opinion on this subject, the directors would call attention to the fact, that in those kingdoms and states where vaccination has been most efficiently carried out by legislative enactments, the mortality from variola has been comparatively the least.*"

In 1857—"The directors take this opportunity of again expressing their opinion of the necessity which exists of some legislative enactment for the efficient carrying out of vaccinations in this portion of the United Kingdom. . .

"The equitable payment of public vaccinators will certainly prove beneficial; but the directors consider such will not go the length required to meet all existing evils. *For, though the public vaccinators will be encouraged to more active exertion, still the apathy and prejudices of the lower orders will not be removed thereby, nor their preference, in many instances, to small-pox inoculation counteracted.*"

And then, after drawing the attention of the Legislature to various desiderata required for the success of a perfect system of vaccination, they suggest what would appear to them most likely to obtain such, as follows:—

"It would appear that all these important desiderata relative to vaccination have been fully attained in Germany by the introduction of a law which compels all parents to produce, on demand, the certificate that their children have been duly vaccinated. In France, although the people are not absolutely enforced to use vaccination, regulations are made by Government, which are in effect tantamount to a legal enactment, making vaccination general among the poor. For example, a rule exists, which prevents the admission of a child into any educational establishment (all of which, both public and private, are under control of Government), who has not a certificate of vaccination.

"The directors are aware that a compulsory act could not at present be introduced into this country; but they are of opinion that Government, by exerting its influence over the National Schools, and thereby instituting a regulation similar to that before alluded to as existing in France, would, to some extent, further the benefits of vaccination.

"The directors are moreover of opinion that if the clergymen of different religious persuasions could be induced to join (as no doubt they would in such a benevolent purpose), much assistance would be afforded to Government in removing the obstacles which, through ignorance and neglect, still exist among the poor, obstructing the spread of vaccination."

Lastly, in the Report for the year ending March, 1859, which made its appearance prior to the last assemblage of the members of the British Association, the directors again direct the attention of the Government, the profession, and the public to the imperfect regulations for vaccination throughout Ireland. They explain how it could be improved, to a certain extent at least, that is, until some legislative enactment should be in force, and most unmistakably recommend the compulsory system, strange to say, in the following words:—

"For England and Scotland there is an act, called 'The

Registration of Births, Deaths, and Marriages Act;' and proper officers are appointed, who, by the help of the Registry, are enabled, in every family, to inquire as to whether each child of three months old has been vaccinated or not. The parents are liable to pay a penalty if vaccination has been omitted in any case, except they can show a certificate from a medical man, stating a sufficient reason why the child has not been subjected to the operation.

"The directors believe they are correct in stating, that Ireland is the only country in Europe in which the people are left entirely to themselves as to vaccination, and are permitted either to neglect or subject their children to the process, as they please. The directors are of opinion, that until an act can be introduced into Ireland, similar to that of England and Scotland, there cannot be carried out any efficient system of punishing, by fine or penalty, those parents or guardians who have neglected to have their children properly vaccinated at an early period; and, in the absence of the power of inflicting some penalty, they fear that rules and regulations will have but partial effect.

" The directors have little doubt but that such an act as the Registration Act, already alluded to, could be extended to Ireland, and that in time most of the evils which at present retard the progress of vaccination in this country might thereby be obviated. But, in the meantime, and as a preparation of the people for the reception of such a new law, the directors are of opinion that much good would be done if Government would use their influence with the Commissioners of the National Education Board, that they should require the parents and guardians of all children obtaining the advantages of the national education, to produce a certificate, stating that such children have been duly vaccinated; a regulation which, even on sanitary grounds, and for the good of those attending the schools, seems necessary. Many of these matters have been already (in their former Annual Reports) laid before the Government.

" The directors feel they have not the power to do more than to point out the evils which they believe to exist, and leave it to those in authority, and to the Legislature, to redress them.

" In conclusion, the directors would reiterate the opinion expressed in their last Report, viz., that if the clergy of every denomination were requested to join (as no doubt they would for such a benevolent purpose) in endeavouring to remove the prejudices existing in the minds of their respective flocks relative to vaccination, much aid would be thereby afforded to the

Government, in causing the protection from small-pox to become universal throughout the country; and the exertions of all persons to aid this object is this year the more earnestly urged, because small-pox, which has been so general throughout England, has lately made its appearance in an epidemic form throughout several localities in Ireland."

We now append the whole gist of Dr. Moore's pamphlet in the following extract, which contains the method he recommends for the perfect vaccination of this country:—

"The Act of August 2nd, 1858, 'To make further provision for the practice of vaccination in Ireland,' sub-divides the dispensary districts into vaccination districts, and gives 20*s.* remuneration to the medical officer for every twenty cases. Now, from personal experience of the working of vaccination, the extent of dispensary districts, and the apathy, or, I may add, the repugnance, in many instances, of the recipients to avail themselves of this boon, I fear this amended Act cannot work satisfactorily; besides, the remuneration to the medical officer is inadequate to stimulate him to any special zeal, cumbered as he is with the onerous duties of a dispensary district. No doubt the measure is an expedient one, all things considered, but till we obtain a base of operation in the shape of a registration of births, deaths, &c., bill, we must be said to be legislating from the wrong direction; in short, building a superstructure without a foundation. As regards the mode of obtaining vaccination returns by capital remuneration for individual cases, I need not enter on the subject on the present occasion; suffice to say, that many and grave objections can be urged against such a system.

"Whether as yet we are ripe for a compulsory vaccination act, or whether it is not deemed sufficiently criminal on our part to throw overboard the preventive and sanitary means at our disposal, I must leave for others to determine; but, in the meantime, till some such happy consummations are attained, let us set about putting our house in order.

"By the Amended Vaccination Act (England) 14th June, 1858, 'the registrar-general shall transmit from time to time to the registrar of births, deaths, &c., in every district, such books and forms as may be requisite for the medical officers appointed as in said act mentioned, and other duly qualified medical practitioners in such districts.'

"Now, let us apply this to Ireland. First, let an act be passed for the registration of births, deaths, and marriages. Secondly, on this, as a base of operation, found vaccination; dissever it 'in toto' from the poor-law system. Vaccination is an institution of far too great importance to be tacked to the tail of any establishment, least of all to one already too cumbrous. There can be no insuperable difficulties in carrying out the details of this measure. Let the kingdom be divided into vaccination districts, over which the most

active surveillance and inspection should be kept by men of the highest professional acquirements, and, ere long, I have no doubt this pestilence would be reckoned among the 'things that were,' and we would attain a status of immunity second to no civilized state in Europe. Such we might regard as an outline of the official view of the question, which, in the absence of a compulsory act, could be negatively and socially backed up, so to speak, not only by the authorities, but by every individual member of society. Thus, let all candidates for public appointments, to the very lowest grade, produce proof of having been duly vaccinated, as a *sine qua non* for their candidature; let all large employers and private individuals make such by-laws in their several establishments, so that vaccination must be indispensable for entering on apprenticeships and services of various kinds; and thus, with every facility afforded, in an independent manner, by the State, I should not fear that this *temporal* would become as familiar, and duly enforced, as that of baptism, or other *spiritual* injunctions."

It would appear probable that the author intended this pamphlet to be one of a long series on "Preventive Medicine," otherwise whence the necessity of marginal notes, answering as heads to the text? In a *volume* of fourteen pages we can scarcely conceive the reader requires such great facilities for reference. But should this *brochure*, from many similarly interesting additions, swell into a ponderous tome, then, as is the case when one wishes to consult some large volume of bound-up old Acts of Parliament, this marginal arrangement would prove of considerable convenience, affording the means of readily obtaining important information upon topics of such interest as might be expected from the author. If such be the intention of Dr. Moore, we would request him to tell us hereafter a few things we do *not* know, and always to look before him previous to his taking a jump at the British Association.

We cannot conclude this notice without saying a few words concerning a letter which has obtained newspaper circulation lately throughout Dublin, concerning—not vaccination, but—our Cow-pock Institution. The tenor and animus of it are obvious; and, as facts which we were positively acquainted with, and which we conceived all the profession were equally cognizant of, do not appear therein, we consider we shall not be out of place here in supplying them.

In this letter, headed "Vaccination and Small-pox in Ireland," the writer contrasted the English with the Irish vaccine establishment, and informed the public and the profession that, at the former, lymph could be procured free of expense, whereas, at the latter, a charge was made for it. This letter was pecu-

liarly adapted to mislead both the public and profession. Whilst it extolled the liberality of the English establishment, and decried what it called the "selfish" trading disposition of the Irish Cow-pock establishment, it lauded the facilities to the spread of vaccine afforded by the one, and bewailed the difficulties thrown in the way of its diffusion by the other.

It is not our intention to occupy space further than by explaining, for the information of the profession and the writer of the letter alluded to (who, actuated, as he doubtless has been, solely by a philanthropic motive, must have been ignorant of the facts of the case, or he would have mentioned them), that there are peculiar circumstances in connexion with the Irish vaccine institution which necessitate a charge for lymph, and that these necessary circumstances are altogether removed from the English establishment. The Dublin Cow-pock Institution—which was founded above fifty years ago, for the free vaccination of all the poorer classes who might wish to apply for that purpose, and for the distribution of recent and pure vaccine lymph to practitioners—has always been, to a great extent, a self-supporting establishment. It is now allowed but *four hundred* pounds annually of the public money; and this grant has but very recently been increased to its present amount, having previously been *one hundred* and *two hundred* at different periods. Were the current expenses deducted, at any time of the institution's existence, from the grant, there would remain no adequate emolument for the medical officers engaged in conducting it. The difference had then to be made up by annual subscriptions from dispensaries, poor-houses, and private practitioners, for the supply of fresh vaccine lymph, whenever their stock happened to run short or die out.

The National Vaccine Establishment (England) is *not* a self-supporting institution; it obtains from Government the sum of, at least, *two thousand pounds* per annum; whereby means are afforded to it, not only of extending the sphere of its action to an extraordinary degree, but also of giving to all applicants lymph free of expense. For the above reason it appears, through the medium of this writer, in a more favourable light before the eyes of the Dublin public than their own Institution; and yet, notwithstanding the great advantages afforded to it by Government, the English Vaccine Establishment, in one of its recent reports, complains of the difficulty it experiences in answering the enormous demands made upon it for lymph.

Some time since, as we have been informed, Government was solicited to place—by an increased grant—the Dublin Cow-pock Institution in a position to follow the example of the English establishment, and give lymph to all, free of charge; but it

was intimated that "no reason was seen why the present system should be altered;" and, on a previous occasion, when a sum was accorded, much less than that which it at present enjoys, it was accompanied with the *direction* that the Institution should be made as self-supporting as possible.

It has also been reported to us that the directors were always anxious to conduct the Irish Institution upon the same principles as the English, and to do away even with subscriptions, but the power was not accorded them.

Although it is not our intention to enter seriatim into the items contained in the letter referred to, yet we cannot conclude without alluding to one subject, which most certainly tends to disseminate an erroneous idea, and to throw a degree of odium upon the Dublin Cow-pock Institution. The writer says—"Amongst a few of the causes to account for the terrible mortality from small-pox in Ireland, I look upon this charge for vaccine as the most prominent of our very selfishness." We presume he *means us to understand* that this selfish charge, made by the directors of the Cow-pock Institution, is amongst the most prominent cause of the terrible mortality from small-pox in Ireland!

What is the truth? The richer or higher classes of society, with few exceptions, avail themselves of vaccination—a charge for lymph has no influence on them—their medical attendant is applied to; he performs the operation, and is, either directly or indirectly, paid for doing so. If the charge for lymph, then, has the effect ascribed to it above, that effect must be apparent only amongst the artisans, labourers, small shopkeepers, and poorer classes; and if this large section of the people is debarred from the blessings of vaccination by the regulations of the Cow-pock Institution, then we agree with his too evident desire—let it be done away with. The provisions, however, made for the vaccination of the poorer classes are most ample. There is not a dispensary in Ireland that is not a *free vaccinating institution*, at which all, *without exception*, who choose to apply, may avail themselves of the saving influence of cow-pock. To exemplify what we wish to demonstrate, let us take Dublin. Independently of the Cow-pock Institution, with its branch at York-street, there are no less than six or seven dispensaries in this city for the *free* vaccination of the public, not to speak of the suburbs. For each of these six dispensaries the sum of *ten shillings and sixpence* is paid annually to the Cow-pock Institution out of the taxes, in order that the Institution may be supplied with fresh lymph whenever it is required. This charge does not come out of the pocket of the

respective medical attendants of these dispensaries—it is the taxed public money, and there has never been a demur at paying it. Under these circumstances, it cannot surely be asserted that the yearly subscriptions to the Cow-pock Institution is a bar to vaccination, and thence the cause of variolous deaths amongst the greater portion of the inhabitants of Dublin—those to whom only a charge could be an impediment, and to whom there really is *no* charge. The facilities for free vaccination afforded to the public are very great in this city, and yet the aggregate of those vaccinated, we understand, is not in proportion to these facilities. It is exactly similar with respect to the country districts. Upon other causes, then, than the regulations of the Dublin Cow-pock Institution does it depend that vaccination has not progressed as it ought to have done amongst the masses—causes, for the most part, already mentioned. The regulations of the Cow-pock Institution have no effect whatever upon the richer sections of society, and the masses are not depending upon the private practitioner for vaccination, but, on the contrary, all can *freely* partake of its blessings.

This half-crown charge for lymph at the Dublin Cow-pock Institution is intended for practitioners who are about to vaccinate private patients. It does not affect the poor, the decent tradesman, the artisan, or those who, though they can afford to pay a practitioner, are too mean to do so; even these can obtain free vaccination for their families. The only one it can affect is the practitioner, and surely, if he is paid for his services, it cannot do him an injury.

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*Lectures on the Development of the Gravid Uterus.* By William O. Priestley, M. D. London: Churchill. 1860. 8vo, pp. 110.

These Lectures were prepared for the pupils of the Grosvenor-place School of Medicine, London, and were originally published in the Medical Times and Gazette. The author was an assistant of Dr. Simpson's, and is known to the profession as joint editor (with Dr. H. R. Storer) of his Obstetric Memoirs and Contributions. The Lectures are eight in number, and give the fullest and clearest account that has, perhaps, ever appeared of the subjects to which they relate. The author seems to be practically familiar with what he writes about, and is peculiarly happy in his style. The illustrations are numerous and effective, and some of them are from original drawings. No opportunity is lost of pointing out the bearing of physiolo-

gical and anatomical facts upon practice; and this feature of the work cannot fail to give it interest to all readers, even to those whose pursuits are wholly practical.

We cannot offer as minute an analysis of these Lectures as they deserve, or as our readers might wish. We can only recommend them to read the work itself, and now lay before them a few extracts. The opinion advanced by William Hunter, that the *membrana decidua* is a uterine product, has been strongly controverted, even by so high an authority as Dr. Robert Lee. The accuracy of Hunter's description, however, receives strong confirmation from Dr. Priestley:—

"That the decidua is an uterine product, and not brought by the ovum, when it first makes its appearance in the uterus, is proved by a variety of circumstances. In the first place, the researches of Baer and Weber have proved that it is present in the uterus in the first week of conception, before the ovum has quitted the Fallopian tube. Secondly, it has been observed to exist in the uterus in a considerable number of cases, in which the ovum never reached its cavity, as in the instances where it was developed in the Fallopian tube, or in some extra-uterine position. Thirdly, it is supplied with nutrient vessels, arising directly from the uterine parietes, while the fœtus and its immediate dependencies have a circulation peculiar to themselves, and apart from the maternal system. Lastly, the similitude the entire membrane bears to the shape of the uterine cavity, and the histological elements which compose its intimate structure, as ascertained by the microscope, seem definitively to settle the question."

Dr. Lee maintains that a decidua is occasionally found surrounding the ovum in extra-uterine pregnancies. Dr. Priestley does not question this fact, nor does he attempt toe xplain it, which we think he might have done. He contents himself with saying, that it "does not affect the proofs of the fact that the mucous membrane is transformed into a decidua whenever the ovum is developed in the uterine cavity."

The author entirely agrees with Dr. Simpson in regarding the membrane occasionally discharged from the uterus in cases of dysmenorrhœa, as being identical in its nature and structure with the decidua; the only difference between them being the cause of production.

In his remarks on the *chorion* the author takes occasion to speak of the disease commonly but incorrectly called uterine hydatids, and he agrees entirely with those pathologists who regard it as a morbid condition of the chorial villi. This indeed is the view generally entertained at the present day. The most important question connected with this disease is, whether it

can ever arise independently of conception. His own conviction is "that the cystic chorion cannot be formed except as the result of impregnation;" and he adds, "but so long as observers differ on the question, it cannot be regarded as finally decided." We fear, then, there is little chance of its ever being decided, even if the evidence were twice as strong as it is; nay more, there is no question in the whole range of medicine which can be considered a *decided* one. This unanimous consent of observers is not to be found on any point. We have seen one or two preparations in which the cysts seemed to be at both sides of the decidua reflexa. Now, we would just ask, is it possible for these vesicular growths to take place in the decidua? If it be admitted that they can, then we have a strong argument in favour of the existence of uterine hydatids independently of impregnation; because we know that a membrane structurally the same as the decidua may be formed in the virgin uterus. Under this aspect, the question is open to fresh investigation.

Respecting that much debated question, the source of the blood in hemorrhage from the gravid uterus, Dr. Priestley adopts the opinion so strongly contended for by Dr. Simpson and a few others, that the blood comes from the detached surface of the placenta, and not directly from the uterus; this, at all events, he says, is the rule, and when the blood comes from the uterine vessels that have been torn across in the separation of the placenta, it is to be regarded in the light of an exceptional occurrence. Further, speaking of placenta previa, he tells us "it is pretty generally understood that when detachment (of the placenta) is complete, hemorrhage, as a rule, ceases—the vascular supply being entirely cut off." Now, with the greatest respect to the talented lecturer, we must express our dissent from these doctrines, even though they are supported by the great authority of Professor Simpson. The question of *the source of the hemorrhage* has been most carefully investigated by Dr. Barnes, and the conclusion at which he arrives is, that the blood comes from the uterine surface; and this is strongly supported by the observations of Dr. Merriman, Dr. Chowne, and Dr. Mackenzie. This same view is the one to which we ourselves strongly incline. Dr. Barnes has also reviewed, with great ability, all the facts and arguments in favour of the practice alluded to in the text, of total separation of the placenta in cases of unavoidable hemorrhage; and as the result he states: "We are then amply justified in concluding, that the artificial total detachment of the placenta cannot be relied upon to arrest the hemorrhage." The fact of most profuse

hemorrhage being of frequent occurrence after the entire exclusion of the after-birth might be sufficient to convince any ordinary observer that flooding to any extent may come from the uterine surface, and that complete detachment of the placenta gives no security against it.

A much agitated question of late has been, whether or how far, the uterus is deprived of its mucous coat by the act of parturition? Very opposite opinions have been expressed upon this point by many able anatomists. Dr. Priestley has given much attention to it himself, and we therefore give the results at which he has arrived:—

"I have little hesitation, therefore, in affirming, that after an ordinary labour the muscular substance is not laid bare, as erroneously asserted by some of the authors quoted; and that the shreds or flocculent processes observed hanging from the inner surface of the uterus are not muscular bundles, but really the remains of the decidua uteri—a stratum of which is left to protect the interior of the cavity until the mucous membrane is reproduced, when it breaks up and passes away in particles with the lochial discharge. So far as my observation goes, the stratum of decidua left on the uterine surface is essentially different in structure from the mucous membrane out of which it was formed originally, being composed of fibres and irregular cells mixed with fat globules, instead of the regular epithelial particles of a mucous lamina. Schroeder van der Kolk has traced the gradual shedding off of this layer of the decidua in the days subsequent to delivery, and he found that the large fibres which entered largely into its composition, and which he was able to distinguish from the uterine muscular fibres by their softer outline, undergo fatty degeneration, and by this process are resolved into small fragments, which mingle with the discharges. The large admixture of blood-globules gives the sanious appearance to the lochiæ in the early days of convalescence; but later, when the blood-globules are less in number, an almost purulent appearance is produced by the cellular and fatty *debris*, resulting from the disintegration of the residuary decidua, and these constitute the chief part of the discharge.

"The mucous membrane of the cervix uteri contributing nothing to the formation of the decidua, is found almost unchanged at the end of gestation."

Here and there in the course of these Lectures the author makes some pathological remarks, just enough to make us wish for more. On different occasions he vindicates the accuracy of William Hunter's observations, and thus speaks of this truly great man:—

"And while speaking of William Hunter, I cannot refrain from pausing for a moment to pay a just tribute to his genius and saga-

city. It is the fashion among scientific men in our own days to extol the name of John Hunter, as one of the greatest men of his time, and well his name deserves all the eulogiums which can be bestowed upon it; but, in looking carefully into the labours of his brother William, one is often tempted to think that, although his pursuits may not have been so varied and extensive as those of his more celebrated brother, yet, for profundity of observation and true sagacity, he was in no respect his inferior."

In conclusion, we must express our obligations to Dr. Priestley for thus placing before us, and in such an agreeable form, the combined results of his reading, reflection, and investigations on a most important anatomical subject. We would suggest, as before hinted at, that should another edition of these Lectures be called for, he would enter at length into the pathology of the different organs and structures of which he speaks. Judging from the specimen now before us, we are quite sure he would do the subject ample justice.

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*On the Diseases and Injuries of the Joints: Clinical and Pathological Observations.* By Thomas Bryant, F. R. C. S. London: Churchill. 1859. Fcap. 8vo, pp. 273.

Mr. Bryant's work is one that requires to be looked at in one of two lights. If considered as a text-book on the subject of which it treats, it falls lamentably short of its object: not for one moment can it bear comparison with any of its compeers on those subjects; but if we are only to consider it as a *contribution* to a subject the literature of which has already had good service done it by previous labourers, we are bound to receive it with gratitude, and to acknowledge its many great and practical merits. But, in whichever point of view we may choose to consider it, we must comment on one fault, one vital omission, unhappily, now-a-days, but too prevalent in our monograph-making literature: it is the vice—we can use no milder term—of not rendering unto Cæsar the things that be Cæsar's, the not referring to the proper authorities the valuable facts and observations with which each special subject has been enriched. In our opinion, an apology, such as we find in the preface, is no valid one, for not doing the trivial justice of recording his exertions on behalf of our science to each *original* investigator of pathological surgery.

"The labours of my predecessors have not been disregarded; and if their names have been left unmentioned, it is not from the want

of a due appreciation of their merits, but from the idea that the record of personal experience is generally the most valued, and that by the adoption of another course these pages would have been indefinitely extended."

Nor is such a line of conduct just to the student who proposes to learn from Mr. Bryant's pages the history of the subject of which he treats; nor can he be expected to enunciate the opinions of such men as Brodie, Smith, Adams, Butcher, &c., if he be not fully taught them. True, he may know them, but he knows them as Mr. Bryant's, not as their originators' views. Such conduct, we repeat, is not just either to the original investigator or to the student: it is a crying, serious, *increasing* evil—one that no plea of expediency can justify, and one that, on the grounds just stated, must be put down.

Well, then, were we to look on Mr. Bryant's work as a full exposition of what is known on "Diseases and Injuries of the Joints," we should dismiss it in a very few lines; we should content ourselves with saying that it aims at that which it is perfectly incapable of realizing. On the other hand, if we are merely to consider it as a contribution from the pen of a gentleman already favourably known to the literature of our profession, as a successful occasional writer on these subjects, we hail its appearance with pleasure: it bears, on every page, the impress of a thoughtful mind, of a scientific surgeon, of a practical man, well read in the literature of his subject, and well up to its successful practice. Mr. Bryant has certainly had many opportunities of witnessing examples of the forms of disease of which he treats, and evidently has profited by his experience; and, rarer gift still, possesses the power of graphically describing to his readers what he himself has learned, and is apparently so thoroughly acquainted with. His cases are well and briefly described. Everything redundant is expunged; all that could illustrate them, all that is essential, is brought into bold relief. Had he indulged himself in fuller details in this respect, his book would have been double the size, but not one whit increased, nay, even diminished, in value. Some of these cases are rich mines of practical surgery, and, no doubt of it, will hereafter be of incalculable advantage in guiding his professional brethren in the management of cases of similar difficulty.

Did we not incline to the opinion that Mr. Bryant's object in writing these pages was rather to afford suggestive hints to his professional brethren in the treatment of confessedly a difficult department of their profession, than to supply our rising generation with a text-book on the subject, we should

feel ourselves constrained more minutely to enter into details, of what we consider, in these respects, his shortcomings. As it is, we shall content ourselves with the more grateful task of directing our readers' attention to the manner in which he has discharged his present duty. Mr. Bryant has divided his work into two parts, the first of which is devoted to the consideration of diseases, the second, of injuries of the joints. The former of these is subdivided into twelve chapters, in which are discussed the following subjects:—Diseases of the synovial membrane; pulpy and gelatiniform diseases of the synovial membrane; diseases of the articular cartilages; diseases of the articular extremities of the bones; tumours on the articular extremities of the bones; disorganization of joints; anchylosis of joints; amputation or excision; on loose bodies in joints; on inflammation of the cellular tissue external to joints, and bursitis; hysterical affections of joints. The observations on the question of excision or amputation of joints are written with much reserve, the author, if anything, rather inclining to the side of the opponents to the operation of excision. Here, in our opinion, Mr. Bryant has particularly shown but scant justice to the illustrious advocates of the operation of excision: the names of Butcher and Price being but incidentally mentioned, whilst the distinguished pioneers of this bold innovation in modern operative conservative surgery are passed over in complete silence. Park, the Moreaus, Mulder, Fricke, Textor, Crampton, Syme, &c., &c., remain unnoticed by Mr. Bryant, as do also Jones, Fergusson, Mackenzie, Erichsen, and many others who, by their abilities and dexterity, have made this a recognised addition to our operative repertoire. Our readers, however, have less reason than others to regret such omissions, as Mr. Butcher, by his most valuable and instructive papers in this Journal, on this and other departments of the science of which he is an enthusiastic votary, has fully filled up any hiatus that otherwise might exist in this department of our profession.

The second portion of Mr. Bryant's work is devoted to the consideration of the injuries of the joints. Under this head we have considered :— Sprains and rupture of ligaments; wounds of joints; compound dislocation of joints; compound fracture into joints; compound dislocation and fracture of the phalangeal joints; simple dislocation of clavicle, of shoulder, of the elbow-joint, of the wrist, of the hip, of the knee, of the ankle, of the bones of the foot. Under all these headings, amongst, of course, much that is trite and well known to our profession, we find many interesting facts and valuable clinical

observations. One new, in addition to our many previously well-known plans for the reduction of dislocation of the head of the humerus into the axilla is here described; its novelty and ingenuity recommend it to the notice of our readers:—

"A method of reduction is exemplified in the next case to be related, which I believe is of some novelty, and is certainly worthy of trial in old cases, where the ordinary attempts fail to reduce the dislocation; its simplicity also is a strong recommendation.

"Case 68.—A man, æt. 32, having, twenty-five days prior to his admission into Guy's under the care of Mr. Cock, fallen out of a cart upon his shoulder, dislocated the head of the humerus downwards. Several attempts at reduction had been made, but without success. When admitted, chloroform was given, and extension at all angles made without benefit; an air pad, made of vulcanized india-rubber, was then placed in the axilla, and the arm firmly bandaged to the side; the air-pad thus being made to exert a powerful outward pressure upon the head of the bone. Upon removing the bandage, upon the third day, the head of the bone was found to have returned to its natural position. The man was unconscious of any sudden snap, which is generally experienced in the reduction of dislocations; and it is probable that, in this case, no such sudden movement had taken place; the regular and steady pressure upon the head of the bone probably pressed upon the capsular ligament, which was the real obstacle to the reduction; and this at last gave way, and permitted the bone gradually to assume its normal position. Whatever way, however, we may wish to account for it, the end was a good one, and the method is worthy of remembrance and repetition in similar cases."

Whilst on this subject of the reduction of dislocations of the head of the humerus into the axilla, we must be permitted to enter our protest against that relic of barbarism, which, however, seems to be the favourite one with Mr. Bryant,—we allude to the plan of reducing it by the heel of the operator placed in the axilla of the patient; from the pressure so produced on the axillary nerves, we have seen most unpleasant consequences long afterwards remaining. We much prefer almost any of the other plans suggested for its reduction, but one in particular, which we remember years ago having seen practised by Mr. Rynd in the wards of the Meath Hospital: the process, as we would now term it, of *manipulation*, by which the operator insinuates his fingers over the head of the humerus into the axillary cavity, and by a series of gentle manœuvres so works it downwards, that its return into the glenoid cavity seems almost to be a work of magic, so effectually, and with such comparative ease and freedom from pain is it effected. We have seen this operation frequently practised by the surgeon alluded

to, and have had on many occasions since to feel grateful for the lessons there learned. There is one other point in connexion with this dislocation as being of clinical importance, on which Mr. Bryant would have done well to insist: we allude to the chance of secondary luxation in patients of advanced years. We remember not long since having been called to see an aged gentleman, the subject of this accident, in whom it had been carefully reduced in the first instance, but who was now complaining of intense pain and œdema in the forearm—all caused by secondary partial luxation, no doubt due to a want of tonicity in the muscles surrounding the shoulder-joint. Re-establishing the bone in its normal position, and a properly adjusted bandage, soon removed all his untoward symptoms.

It would be indeed a fortunate circumstance for our rising class of medical students, if all that is essential about dislocations of the hip-joint, could be compressed into the few pages devoted to the subject by Mr. Bryant. However meagre be his remarks on this most formidable class of dislocations, still we find something to extract, though it be the production of another's pen—it is M. Nélaton's formula for ascertaining the position of the head of the femur—

"It has been well pointed out by M. Nélaton, that the best method of determining the exact position of the head of the femur consists in defining accurately its position relative to a line drawn from the anterior superior spinous process of the ilium to the most projecting portion of the tuberosity of the ischium. In its normal position the great trochanter just touches this line, and this junction is maintained in any natural position of the limb; if then in injuries or diseases of the joint, when any doubt exists as to the fact of dislocation, the trochanter is found in this its normal situation, the probability of a displacement is much diminished; if, upon the other hand, it is found to project above or behind this line, there is strong reason to believe that some dislocation has taken place."

Mr. Bryant's observations on our most irrational method of describing dislocations of the ankle-joint, are worthy of serious attention; though we doubt much that, for some years to come, they will receive the consideration which their justice merits. We insert them for the purpose of bringing the question more fully before our readers' notice.

"The majority of authors who treat of dislocations of a joint agree in one thing, and that is, to describe the injury as dislocation of the most movable and distant portion of the articulation. When the shoulder is the joint affected, it is dislocation of the head of the humerus; when the elbow, of the ulna and radius; when the hip, he head of the femur is described as being dislocated; and when the

knee, the tibia and fibula; and yet, when they describe the dislocations at the ankle-joint, they talk about dislocation of the tibia and fibula, and not of the ankle or foot.

"It is always a subject of regret when any generally received arrangement, or method of description, is materially altered, as it tends to confuse both writers and readers; but when an exception to a general rule is made only from habit, as appears to be the case in this instance, and when experience has proved how puzzling this departure from the ordinary method is to students, I do not hesitate in the following pages to apply the same method of description to the dislocation of the ankle that has been pursued in other articulations, believing that the change is decidedly in favour of a clear understanding of the subject."

We have now drawn our readers' attention to some of the more important of the subjects discussed in Mr. Bryant's work. One class of joints has been entirely omitted by him, both as regards its injuries and diseases: we allude to those of the spinal column; why this should be so, we cannot divine; it cannot be that they have not fallen under his notice, or, having done so, that he considers them unworthy of observation. But these and all other deficiencies, we doubt not, we shall see fully rectified in a work whose merits must, and that soon, command a second edition. In the meantime we recommend this one to the careful perusal and consideration of all who value observation, judgment, and experience, applied to the treatment of a most important class of surgical diseases.

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*A Practical Treatise on the Diagnosis, Pathology, and Treatment of Diseases of the Heart.* By AUSTIN FLINT, M. D. Philadelphia: Blanchard and Lea. 1859. pp. 473.

This book is very creditable to an author with whom we have already made a favourable acquaintance, and whose reputation will be enhanced by its publication. It is divided into ten chapters, arranged somewhat differently from the order in vogue, the earlier divisions being devoted to the discussion of enlargement of the heart, and lesions affecting its walls, followed by a chapter on lesions of the orifices and valves,—the work being concluded by a view of the results of inflammation and a summary of functional maladies affecting this organ. This arrangement has decided merits: among others, the prime one of bringing early under the notice of the student the general characters and relations of disorders of the heart, in so far as they affect the capacity of its cavities or the condition of

its walls, including under this latter head over-growth, under-growth, and degeneration of tissue. When it is considered that the greater or less tendency of heart diseases to shorten life, either by a sudden syncope or by slow impairment of vital functions, and that the proclivity to the production of the great variety of secondary maladies which are apt to attend upon disorders of the central circulatory organ, are far more closely connected with the conditions just named than with the mere existence of valvular and inflammatory lesions, the reader will see the value of Dr. Flint's arrangement, as being useful, though not after the strict order of the natural history of the diseases described. In fact, in his first chapter the author is fain to admit that practically the diagnosis of simple hypertrophy, apart from valvular diseases and similar lesions, "can hardly be said to be established by observation. The symptomatic phenomena which are described as distinctive of it have been determined inferentially rather than by facts observed in well authenticated cases."

There is a tolerably complete account of the mode of production of hypertrophy. But it must be confessed that Dr. Flint does not appear to estimate fully the great difficulties which still surround the explanation of this state in certain cases, and cuts from under him the only feasible solution of some cases of hypertrophy unattended with valvular lesions. Thus, alluding to obstruction in the systemic veins, it is observed that "it has been conjectured that in this way Bright's disease of the kidneys may lead to cardiac disease, the affections being not unfrequently associated. It is, however, a question whether, in such instances, the affection of the kidneys be not consecutive to, and dependent upon, the affection of the heart." Surely here the present state of science is scarcely adequately represented, for recent statistical and clinical inquiry has placed it beyond a doubt, that renal maladies lead to the production of cardiac hypertrophy. This much may be considered assured against future controversy; but as to the precise method in which a diseased kidney reacts upon the circulating forces, existing knowledge supplies us only with the wildest hypotheses, and plainly unequal to account for the thing to be explained. We know nothing more remarkable than the universal assent which has been given to the doctrine that the presence of urea in the blood is the cause of the heart's over-growth—a fancy for which there is not the most remote basis either in physiology or pathology, and which is directly opposed to the well-known fact, that in a large per-centage of cases of Bright's disease, even of long standing, there is no at-

tendant hypertrophy. It is sufficiently note-worthy that the amount of repletion of the vessels, as a positive resistance to be overcome by the heart, has never been alluded to in this aspect, namely, as capable, if excessive, temporarily or permanently, of engendering increased growth of the heart's walls. Some cases of Bright's disease are permanently attended with diminution of aqueous secretion, both from the skin and kidneys; others are at various periods so affected; others again appear throughout to excrete even an abnormal quantity of fluid. How different must be the action of the diseased organ upon the motive centre of the circulation, in such diversified states of the fluid contents of the vascular system! If it be not so, the imposing science of hydraulics, so often brought in to illustrate the fluid dynamics of the animal body, requires very important amendments; for we have been taught that in closed circles the motive power must be as the resistance, and that the resistance is made up of friction, ratio of size and length of vessels, and quantity of fluid. The former of these elements of the equation may be practically looked upon as constant in the animal system, excluding very marked changes in the physical properties of the blood; the last, or the amount of fluid pressure on the walls of the vessels, and consequent reaction on the *vis a tergo*, must vary every hour of the day. Such variations may still remain within physiological limits; but if those organs, one of whose most important offices is the elimination of fluid from the system in certain quantity, be permanently deranged, they will pass these limits, and a corresponding change in the central forces of the circulation sooner or later necessarily ensues.

In reference to cases of simple hypertrophy, it is stated that their occurrence is so rare that they may be classed among the curiosities of medical experience.

"The best specimen of simple, uncomplicated hypertrophy which has come under my observation, was obtained at the autopsy of a young unmarried female, who died after an abortion had been procured, in the latter part of pregnancy, by a practitioner of homœopathy, who was convicted of the crime, and sent to the state prison. This female had apparently been well and vigorous until her pregnancy, when she became anasarcous. The kidneys presented evidence of granular degeneration. Death occurred just after delivery, during a convulsion. The heart weighed in this case fourteen and a half ounces; the thickness of the left ventricle was nearly an inch, and that of the right ventricle a fourth of an inch. None of the cavities appeared to be enlarged. Nothing was developed in the judicial investigation of the case to show that there had existed symptoms referable directly to the cardiac hypertrophy."

Now, in this example two very efficient causes of hypertrophy co-existed—first, the granular kidneys, performing their functions so imperfectly that the subject became anasarcous; secondly, the particular condition of the vascular system attendant upon pregnancy. The author, though usually very well informed, is evidently unaware of the important researches which were made many years ago in Paris, chiefly at the Maternitè, upon this subject. Dr. Larcher[a] concludes that the heart is normally hypertrophied during the course of gestation—the thickness of the left ventricle becoming augmented at the least to a fourth, at the most to a third, and being thicker, firmer, and of a more lively red than normal. These observations are based upon 130 subjects who died in 1826-7, soon after delivery. M. Jacquemier in 1837 seems to have made somewhat similar observations. The same points were subsequently (in 1846) determined by M. Ducrest, at the instigation of that able physician, M. Beau, who concluded from these researches "that the heart of women was normally affected with hypertrophy during pregnancy." Now, plethora among pregnant females, as is justly stated in the memoir of Larcher, is not a simple accident, but rather a constant phenomenon—a necessary physiological condition. Hence we see that there were very good reasons for the production of simple over-growth of the heart in Dr. Flint's case. The poor woman was vigorous up to her pregnancy; her kidneys became affected with granular degeneration, and she died immediately after delivery[b].

The opinion of Corvisart, for the most part indorsed by his followers, that prolonged functional disorder frequently eventuates in the development of hypertrophy, is not, according to Dr. Flint, sustained by clinical experience. We are not disposed to agree with this statement, though, at first sight, inconsistent with the fact that the abnormal growth of the muscular walls of the heart is the consequence of abnormal muscular action of the organ; but—

"This inconsistency disappears when it is considered that functional palpitation, even when intense, does not involve that increase of power or strength of muscular action which is incident to the over-accumulation of blood from an impediment to the circulation.

[a] "De l'hypertrophie normale du cœur pendant la grossesse, et de son importance pathogènique. Par le Dr. Larcher. Archives Generales de Médecine." Mars, 1859.

[b] We think it just to take this opportunity to bring under the notice of our readers the important Essay of Dr. L. Traube, of Berlin, "über den Zusammenhang von Herz- und Nieren-Krankheiten. 1856." A truly masterly analysis of the subject above discussed, and the perusal of which has confirmed opinions previously arrived at by us from independent observation.

Moreover, the increased action from nervous excitation is never so constant and persistent as that due to valvular or other lesions which occasion obstruction. In the latter case, hypertrophy is the result of increased action, beginning imperceptibly, and progressively increasing for many months, and even years."

The most striking feature of functional excitement of the heart is the want of correspondence between the complaints of the patient and the observations of the physician. So marked is the hyper-neurotic susceptibility of such persons, that even the normal action of the heart is attended with pain and the sensation of palpitation. An exact counterpart to this is sometimes noted among the poisonous effects of tobacco—one of the most unpleasant of these is the sensation of extreme palpitation combined with præcordial oppression; yet to the hand of an observer or of the subject himself, the heart's beat is scarcely perceptible. In these, and in all similar cases, the organ is irritated, and more or less disordered in its physiological function; but it is not stimulated to the orderly, constantly increasing, and imperiously necessary over-action which results, in a compensating direction, from a persistent augmentation of the natural resistances offered by the vascular system. The latest and ablest German authority on these questions speaks with a judicious vagueness upon the point. After alluding to the ordinary causes of hypertrophy, he observes, that "in other cases a permanently exaggerated innervation may lead to hypertrophy. Albeit this can by no means be established with certainty, yet must the possibility not be entirely denied, that oft-repeated excitations of the heart's action (nervous palpitations), whether of central origin, or coming to pass in a reflex manner, can lead to hypertrophy"[a].

The observations on the physical signs of enlargement and hypertrophy are minute, but not more so than the importance of the subject demands. Great pains have been taken to lay down, with precision, the anatomical relations of the heart, and especially of the apex-beat, the determination of the altered situation and extent of which is so necessary in any attempt at an exact diagnosis of diseased conditions. Of twenty-five healthy persons examined, none presented an exception to the rule, that the apex-beat was observed, in the sitting posture, in the fifth intercostal space. In this intercostal space, the im-

[a] "Lehrbuch der Krankheiten des Herzens von Dr. H. Bamberger." Wien: 1857. This author allows that an absolute increase of the weight to be moved (true plethora) could excite hypertrophy, if this condition, in so high a grade, were not doubtful. But experience and probability are opposed to such doubts.

pulse is felt over an area varying from half an inch to an inch and a half in breadth. The average transverse diameter of this area, in thirteen persons, was a fraction over an inch. The centre of this area, where the force of the beat is greatest, is situated within a vertical line, passing through the nipple, at a distance from that line varying from two inches to three-eighths of an inch: the average in eighteen persons being a fraction ($\frac{1}{3}$) over an inch. The distance from the median line to the centre of this area varies between three inches and five-eighths and two inches and five-eighths: the average, in fifteen persons, being a fraction ($\frac{1}{13}$) under three inches. Measured from a transverse line passing through the nipple, the distance varies from an inch and an eighth to two inches: the average, in eight persons, being a fraction ($\frac{1}{7}$) over an inch and a half. These are the relations of the apex-beat in the sitting posture. We believe these measurements to be, in the main, correct, and we quote them because the deviations of the apex-beat, both in the lateral and vertical directions, are all-important in reference to the diagnosis of pericardial effusion, changes of volume, and dislocations of the heart. In reference to the mechanism of the heart's impulse, Dr. Flint refuses to bow down to Beau's theory, that it takes place during the diastole, but dissents from the common belief in this country, that the heart is shortened by an approximation of the apex towards the base during the period of systole. The opinion of the author, that the apex of the heart is elongated during the ventricular systole, is based, apparently, very much on the experiments of his countryman, Dr. Dalton, whose name we cannot mention here without the expression of our admiration for his valuable contributions to physiological science. But, as Dr. Flint observes, this view was entertained by Vesalius, Harvey, and Hunter. Almost all British writers follow Hope's description of the shortening of the heart during its contraction; but it is impossible to read Dalton's[a] remarks, and the details of his experiments, without concluding, with him, that this is an error; amply palliated, it must be confessed, by the great difficulty of the investigation. Some idea of this may be obtained from the remarkable circumstance mentioned by Dalton, that on exposing the chest of a living animal, it is difficult, at first, to determine what period in the heart's pulsation corresponds to contraction, and what to relaxation, of the organ. The method adopted by Dr. Dalton for getting rid of this primary embar-

[a] A Treatise on Human Physiology. By John Dalton, Jun., M.D. Philadelphia 1859. Page 240. Reviewed in our last Number.

rassment was the passing of a small silver canula directly through the parietes of the left ventricle into its cavity. The blood is then driven from the external orifice of the canula in interrupted jets; each jet indicating the time at which the ventricle contracts upon its contents. The canula is then withdrawn, and the different muscular layers of the ventricular walls, crossing each other obliquely, close the opening, so that there is little or no subsequent hemorrhage[a]. Dr. Flint objects to Gutbrod's recoil theory, which we consider utterly untenable.

The consideration of the abnormal modifications of the heart-sounds is preceded by a brief exposition of the mechanism of those sounds in health, and we are glad to observe that the author defends the view, that the first sound is mainly valvular, dependent on the tension of the auriculo-ventricular valves. The increasing unanimity, both among physiologists and clinical observers, upon this point is worthy of note. It is rightly declared that mere exaggeration of the first sound is by no means, in itself, significant of hypertrophy; and, on the other hand, that an abnormal intensity of the aortic, or the pulmonary sound, separately, may become a sign of hypertrophy, affecting, in the one instance, the left, and, in the other case, the right ventricle. The intensity, in fact, of the second sound over those valves respectively must, in a healthy condition of the coats of the aorta and pulmonary artery, be directly as the vigour of the systolic effort. The summary of the whole signs of hypertrophy is complete, and admirably condensed.

In the remarks upon the treatment of hypertrophy, Dr. Flint shows that he has fallen into the common error in reference to the views of Valsalva and Albertini, though the excellent little brochure of Dr. Brady[b] should have aided him in escaping from this mistake. There can no longer be a doubt that the treatment actually adopted by those eminent Italian physicians was most judicious; whereas that handed down to us by the historians of medicine deserves all the condemnation which has been lavished so freely upon it. The indications for treatment are classed under three heads:—1st, to prevent or limit, as far as practicable, impediment to the circulation, dependent on valvular lesions or other conditions, and giving rise to hypertrophy; 2nd, to obviate, as far as possible, weakness of the heart and a tendency to dilatation; 3rd, to quiet undue

[a] Op. cit., p. 239.

[b] On the Treatment of Internal Aneurism by the method of Valsalva. Dublin, 1859.

excitement and irregular action. The author's conclusions in relation to blood-letting are so discriminating that we must quote them *in extenso:*—

"Bloodletting is permissible when there exists over-repletion of the general vascular system, the object being, by lessening the mas of blood, to facilitate its circulation. This object should be clearly understood. It is easy to understand that if the vessels are abnormally full of blood, an irremediable impediment to the circulation is likely to occasion greater accumulation in the heart and its cavities than when the mass of blood to be circulated does not exceed the normal amount. The existence of plethora furnishes the indication for bloodletting, and the removal of this state constitutes the limit to which it may with propriety be carried. Carried beyond this limit, the detraction of blood can hardly fail to be pernicious. It is to be borne in mind that bloodletting is not to be practised because hypertrophy exists, but because over-repletion of the vascular system, added to an existing permanent impediment to the circulation, increases the necessity, as it were, for the production of hypertrophy. Injudiciously practised, bloodletting is injurious in proportion as it impoverishes the blood and weakens the muscular power of the heart. Resorted to with reference to the object just stated, it is indicated in only a certain proportion of cases, and the abstraction of a large quantity of blood is very rarely, if ever, called for. The end for which bloodletting is employed may generally be fulfilled by other methods of depletion which involve less risk of doing harm."

The great object to be ever kept in view in such cases is the warding off as long as possible the supervention of *predominant dilatation*—for as soon as the cavities of the heart permit of decided and frequently repeated accumulation of blood, this untoward condition commences to develop itself, with all the gloomy general phenomena connected therewith. To this end many means are employed, and the value of moderate muscular exercise, as mentioned by Dr. Flint, deserves a high place among those means. It is remarkable that men engaged in the most arduous labours of a physical kind are yet apparently scarcely affected, in a prejudicial sense, by their occupation. In fact, *regular and complete contractions* are the fundamental condition of the maintenance of the proper state of the vascular system, including, especially, the normal tension of the arterial and venous walls; and it is obvious that exercise of a moderate kind, and when within the limits of fatigue, must tend to this result. But nothing can be more certain than that all our efforts to obviate extreme dilatation will be futile if we neglect to keep down the quantity of fluid to be circulated. Here we have a most precious means of combating the worst results of heart-disease deserving of the most minute at-

tention. The striking effects of a hydragogue purgative are sufficiently well known, but the principle on which its beneficial operation depends has been either lost sight of altogether, or, when recognised, has failed to inspire the observer with a more comprehensive knowledge of the rationale of the Therapeia than he previously possessed. Still more remarkable are the results of moderate leeching over the præcordial region from time to time, because they are obtained at the expenditure of so small an amount of material. In this case the relief is not confined to the increased regularity and greater force of the heart's pulsations, or even to the decided improvement in the several secondary phenomena of venous distention, but the sense of comfort obtained by the patient, of freedom from pain and the distressing feeling of combined oppression and fainting, is equally to be observed. Even in cases of predominant hypertrophy, without dropsy or reflex venous currents, or severe local congestions, when the pain in the intercostal spaces of the præcordial region, or about the shoulder and arm, is a marked phenomenon, no mode of relief approaches that derived from the application of a few leeches in the region of the left nipple.

The subject of dilatation is well discussed, and its treatment carefully related. This latter, as far as regards diet, may indeed be almost summed up in one sentence of the author's: "it should be as highly nutritious as possible, and the quantity of liquid ingesta as small as is compatible with comfort."

In the second chapter we find lesions, exclusive of enlargement, affecting the walls of the heart, brought under review—commencing with "atrophy with diminished bulk." We are rightly told that this is a very rare organic affection apart from other morbid states, with which it, as it were, harmonizes. The conditions generally giving origin to it are diminution of the mass of blood, and of its nutritive materials—conditions necessarily involving diminished exertion of the muscular power of the organ. It is observed in tuberculosis, and especially, as remarked by Lebert, in cases of cancer. In relation to its causation by pericardial adhesions, Dr. Flint merely says that "it is said" to follow these; but the observations of Dr. Stokes and Dr. Gairdner might have induced him to speak a little more firmly on the point. Albeit, further inquiry is here most desirable.

The section on fatty growth and degeneration contains nothing new, and is scarcely worthy of the importance of the subject; though, perhaps, equal to the wants of those for whom the

book is especially designed. The admirable researches of Stokes and Quain have thoroughly familiarized physicians in this country with the phenomena and symptoms of fatty degeneration, and Dr. Flint has mainly followed these authors. In a case of uncomplicated fatty degeneration, we are informed that—

"Percussion shows moderate or no increase of the volume of the heart. This is an important negative point. The apex-beat, if felt, will be but little, if at all removed from its normal situation. The beat, if felt, is abnormally feeble, and it will be inappreciable if the heart is greatly weakened. Impulses elsewhere than over the apex will in general not be discoverable. Inspection may disclose a very feeble movement over the apex, or none whatever. The diminished force or suppression of the apex-beat will depend, of course, on the extent to which the left ventricle is affected. The sounds of the heart are weakened. The first sound more than the second shows abnormal weakness. It is also short and valvular, resembling in these respects the second sound. The first sound may be wanting, the second sound being still heard; and, finally, both sounds may be extinct. The latter obtains in cases of a very great degree of fatty degeneration."

Important as are these remarks, it must be confessed that the physical diagnosis of this lesion is at present in a most unsatisfactory condition; so that practically we obtain more insight, at all events an earlier glimpse of the morbid change from an attentive examination of the patient's history and of subjective phenomena. We rely much more upon the mere existence of *panting* on moderate exercise, combined with an occasional complaint of præcordial anguish, and a peculiarly anxious look, with pallid prolabia—if there be no positive evidence of pulmonary or the common cardiac diseases—than upon any or all of the physical signs above enumerated. The pulse is a most fallacious guide, for, though one abnormally slow is undoubtedly noteworthy in this connexion, it is a species of pulse very common in the later periods of life, when fatty degeneration, to a marked degree, is most likely to exist. On the other hand, the frequent, irregular, and small pulse, still more commonly observed, is even more characteristic of mitral disease, or of predominant dilatation. The final remark of the author—"that the probability of the coexistence of this lesion with other affections which the physician is called upon to treat, is important to be taken into account in the interpretation of symptomatic phenomena, and the employment of therapeutic measures," deserves to be carefully kept in mind.

The following section on softening is based mainly on the

researches of Dr. Stokes, relative to the condition of the typhus heart. It is truly observed that the signs are essentially those which belong to fatty degeneration; but it is not known to the author that the structure of the muscular fibre in this condition undergoes a molecular degeneration, with loss of the normal transverse striation, closely analogous to that known to take place in the former disease. Twelve years ago we had excellent opportunities of investigating this point, and our inquiries led us to the conclusion that a rapid destruction of the proper sarcous elements took place in the typhus-softening of the heart; but a change of residence brought these researches to an abrupt termination. We are surprised that so interesting a branch of pathological investigation has not hitherto attracted the notice of histologists possessing adequate means for the solution of a problem, at present practically unworked. All doubts would then be finally set at rest as to the reality of the change undergone in this condition. One remark resting on first-rate authority, to the effect that the reduction of the pulse in frequency below the normal average, at the time of convalescence, denotes that softening has taken place, should be confronted with the fact, that while evidences of these changes are generally wanting in the relapsing fever, the pulse in this form is frequently lowered during convalescence far below the average. This circumstance, however, by no means negatives the general importance of the above cited remark.

After a brief discussion concerning induration, rupture, and cardiac aneurism, we arrive at lesions affecting the valves and orifices of the heart, the symptoms and pathological effects of which are very fully treated of. Among those connected with the heart itself, first comes its enlargement.

"Its amount, as a whole, varies greatly in different cases, and, what is remarkable, is not proportionate to the amount of obstruction or regurgitation, a fact which shows the influence of causes subsidiary to the valvular lesions. Very great enlargement is found associated with lesions involving only moderate obstruction or regurgitation; and, on the other hand, the heart is sometimes found to be but little, or not at all enlarged, when there exists a marked degree of contraction or insufficiency. The mitral orifice has been reduced to the size of a crow's quill, without notable enlargement of any of the cavities. This fact also shows the importance of causes superadded to valvular lesions."

Previous authors have, for the most part, been satisfied with the assertion that the enlargement of the heart bears in all cases, and, as if necessarily, a definite proportion to the valvular disorder. We are glad to find that Dr. Flint recognises the

contrary as being the fact. Its explanation is to be found in the varying states of the whole economy. We again remark that we have to consider here not merely the resistances to the propelling force seated at or near the orifices of exit, but those less definite and more recondite, which are related to the mass of circulating fluids, and the tension of the capillaries in important eliminating organs. We imagine the author, in declaring that obstruction of the coronary arteries is to be included among the *pathological effects* of valvular lesion, intended to write the *concurring circumstances.* In his observations on the pulse we find a rather unsatisfactory account of that attendant upon aortic patency, and are disposed to disagree with the averment that the pulse is sometimes jerking in mitral obstruction and regurgitation. We equally dissent from Dr. Flint's theory of the causation of visible pulsation—"that it is an effect of the collision of the retrograde diastolic current, and the onward systolic current in the vessels," though he also admits that the locomotion of the arteries has something to do with the phenomenon. That this character of pulse, so well described by Dr. Corrigan, is ever observed "in a marked degree in thin persons when the arteries have become rigid or calcareous, with or without cardiac disease," we entirely deny, if the author excludes disease of the aorta leading to its dilatation. Here we note the most curious omission in the book—which displays a thorough acquaintance with the researches of the late lamented Dr. Bellingham, but is without reference to his interesting researches on the diagnosis of dilatation of the aorta. Why, too, is nothing said of the largeness, deceptive fulness, and compressibility of the aortic pulses? and in a book intended for students, we may justly remark on the absence of all allusion to the similarity of this pulse to that existing in some cases of profound anemia. After bringing before his readers venous turgescence and pulsation, Dr. Flint then passes in review the symptoms and pathological effects referable to the circulation, discussing briefly the important subject of embolia: those referable to the respiratory system, the nervous, digestive, and genito-urinary systems; and, finally, the signs to be drawn from the countenance and external aspect of the body. All these sections are most judiciously arranged for the information of the learner, and give us room for unqualified praise only. In the fourth chapter we are presented with a tableau of the physical signs, diagnosis, and treatment of valvular lesions—the "pons asinorum" of the subject. The question is propounded, "Are valvular lesions which involve either obstruction or regurgitation, or both, uniformly or generally accompanied by mur-

mur?" and it is justly remarked that the question may be answered affirmatively as regards the *general*, but not as regards the *uniform* occurrence of murmur. Valvular lesions sometimes exist, the conditions being absent which produce abnormal murmur. Among these, perhaps, enfeebled action of the heart may take the first place, which is well illustrated by the following case, which came under our observation many years ago:—Jane Coleman, aged 25, admitted into the Meath Hospital, Nov. 14, 1846; labouring under fever, which had commenced six days previously. She is unaware of having been exposed to the contagion of typhus. Has had three attacks of rheumatism, and had been under the care of Dr. Graves seven or eight years before with heart symptoms. She has only left the hospital three weeks, having entered it for an attack of rheumatism. During the time of her stay a murmur was observed, soft, but distinct, over the base of the heart, accompanying the first sound. There was no change in this up to the time of her leaving the hospital. She is now copiously maculated over the chest; maculæ large, of a red colour, tongue dry; eyes suffused. She is very weak; pulse 120, weak. Loose rales evident posteriorly over both lungs on her taking a deep inspiration. The first sound of the heart not quite pure, but scarcely can it be said that there is a bruit.

Nov. 15. Slight cough; complains of sickness; pulse 126; not the slightest evidence of bruit.

16th. Pulse 126, weak; lies on the back; vomiting of a bilious matter; intense bronchitis; bowels confined.

17th. Patient very weak; impulse of the heart slight; the sounds nearly approximated in character.

18th. Pulse lowered four beats; appears improved.

19th. Impulse of the heart still weak.

22nd. Countenance improved; slept well; tongue moist; breathes quickly.

23rd. Respiration much hurried; most extensive diffusion of mucous rales behind; pulse very rapid.

24th. Respiration not so hurried, but the rales are intense over the anterior part of the chest; tongue moist.

25th. Breathes easily, and is evidently much improved, and stronger.

26th. Pulse 100. From this time the patient rapidly recovered, but, as she improved, the systolic murmur daily increased, until it acquired the same characters as before she was attacked with the fever. Twelve months afterwards this girl returned to the hospital for the purpose of having the heart examined, and we observed the murmur was becoming more and

more pronounced, and of a harsher character[a]. This is the only case we have seen reported where the signs of typhus softening followed upon a murmur whose cause was ascribable to an organic origin, and it supplies a significant argument on behalf of the reality of that peculiar change in the muscular structure.

Dr. Flint discusses with great acumen the circumstances which, taken in connexion with a murmur, enable the auscultator to determine whether existing lesions involve either obstruction or regurgitation, or both. This most important question is scarcely even mentioned in some systematic works, but the seriousness of an abnormal murmur is absolutely connected with the circumstance that it indicates one or other of these conditions. The difficulties surrounding the determination of this question are very considerable. A systolic murmur having been determined to arise from the mitral orifice, should the murmur be diffused over the left side, extending to the lateral surface of the chest,—actual regurgitation, rather than lesions impairing the normal smoothness of the mitral curtains, ought to be, according to Dr. Flint, assumed. But this guide is, we are convinced, most fallacious, as well as the greater relative intensity of the pulmonic second sound, though this latter deserves far more attention than the former. Any cause productive of tension of the pulmonary system of vessels can intensify the pulmonic sound, but the most frequent cause connected with cardiac maladies is, beyond compare, mitral regurgitation: hence the value of the sign.

It is observed, to our great wonder, that if the aortic second sound retain its normal intensity and purity, it shows that the aortic valve is competent to fulfil its function, a fact which warrants the exclusion of lesions affecting it sufficiently to give rise to obstruction (page 191). It cannot be denied that in a large proportion of the cases of obstructive lesion at the aortic orifice, the valves are so involved in the malady that the purity of the second sound is diminished, or even the whole sound abolished; but surely Dr. Flint must have become clinically cognisant of the fact that there is not unfrequently serious contraction of the aortic orifice, producing marked obstruction and hypertrophy of the left ventricle, the aortic second sound remaining intact. We do not even think that the non-diffusion of a diastolic aortic bruit over the body of the heart, when unaccompanied by regurgitation, as when arising from

[a] An Essay on certain Morbid Conditions of the Heart occurring during the course of Fever. MS. [Rev.]

diseased arterial coats, will serve as a reliable diagnostic in contrast with the phenomena of a true aortic regurgitant murmur. We are, in fact, compelled to admit that the solution of this and such like questions is to be found rather in the general secondary symptoms attendant upon cardiac lesions than in positive differential marks derivable from the examination of the sounds of the heart. If there be no decided enlargement, and no disorder of any of the great functions of life, we may be well assured that the abnormal murmur is as yet unattended either with regurgitation or obstruction. We think that the statement of "some writers," criticized by our author, that the intensity of an aortic regurgitant murmur is greatest at the base of the heart, rather than over its body, as contended by him, is the correct representation of the fact.

In reference to the pathological import of organic endocardial murmurs, the author carefully reminds the reader that it is to be borne in mind that lesions which are innocuous as regards any immediate effects, i.e., which do not occasion either obstruction or regurgitation, may give rise to murmurs; the physical conditions for audible sonorous vibrations being supplied by fibrinous or calcareous deposits on the valves, orifices, or within the aorta. Serious lesions are by no means, therefore, to be predicated on the existence of a murmur which is undoubtedly of organic origin; a point which is again insisted upon in the following section.

We are unable to give unqualified assent to the statement, that an inorganic murmur is *uniformly* soft,—however true, in general terms, it may be that intensity offers presumptive grounds for the belief that a given murmur is of organic origin. But Dr. Flint accurately avers that, as a rule, every persistent, constant murmur referable to the mitral orifice, denotes organic lesion of some kind, and, as a rule, organic murmurs are constant and persistent, while inorganic murmurs, wherever produced, are fluctuating and variable, being sometimes discoverable only when the body is in a certain position. However, the essential point to remember consists in the fact, that organic murmurs, in the great majority of the cases of chronic disease, are associated with more or less cardiac enlargement. The general summary of the diagnostic characters of lesions affecting the mitral, aortic, tricuspid, and pulmonic valves or orifices, is admirable: but we pass it over without further comment, and accompany our guide to the consideration of the treatment of these lesions. This section, within the narrow limits of twelve pages, presents us with a view of the subject at once

exceedingly well written and remarkably complete. Among the preliminary considerations we find the following:—

"The existence of a cardiac murmur, dependent on innocuous lesions, is often ascertained by accident, there being no symptoms of disease referable to the heart. I have repeatedly met with it in examining persons who considered themselves in perfect health. These persons are in no immediate danger, and it would give rise to needless alarm to inform them that they have an organic affection of the heart, since it is a common notion that any such affection involves liability to sudden death. There is, however, a prospective danger not to be overlooked. It has occurred to me, in two instances, to examine for life insurance persons presenting an organic murmur, without other evidence of cardiac or other disease, who were, of course, not deemed proper subjects for insurance; but they succeeded in obtaining policies in other companies, and both have since died with well-marked disease of the heart."

These judicious cautions are well understood in this country, and have been often insisted on. We are acquainted with a gentleman in extensive medical practice, and whose health is more than ordinarily robust, in whom we detected, three years ago, two intense aortic bellows murmurs. We had occasion to note, a few months ago, an aortic murmur in the chest of a gentleman in excellent health, who applied to a friend for the purpose of effecting an insurance policy, and who experienced the utmost alarm at the care with which the chest was examined, protesting a perfect freedom from every complaint. There is one circumstance of great importance not alluded to by Dr. Flint—the extreme comparative frequency of sudden death in cases of aortic patency. There can be no doubt that ordinary valvular lesions, apart from those contingent events which rapidly cut off the patient, such as fibrinous coagula, angina pectoris or rupture, very rarely, indeed, produce what can be fairly termed a sudden death. The ordinary phenomena of retrograde vascular distention in many organs are first developed, and sometimes take place over a long course of years before the terminus is arrived at. Not so with aortic regurgitation, in which form we have seen sudden death occur so often without previous warnings, such as we have indicated, that we never fail to inform the friends of the apprehensions which exist in our mind on this score.

The author dilates on the value of digitalis in the treatment of these lesions and their effects, and we are disposed to think that this remedy has fallen into unmerited disuse in these countries, after having been abused in the other direction to an

extent, which can only be fully credited by those enjoying the opportunity of conversing with some of our older physicians on the treatment of cardiac maladies forty or fifty years ago, when we fear that the medical attendant was responsible, not unfrequently, for the abrupt termination of his case. We have attentively watched the operation of this drug, especially in cases of mitral regurgitation with irregular rhythm and great venous turgescence, and find every reason to believe, with Dr. Flint, that, observing proper caution in its administration, the action of the heart not only becomes more regular under its use, but the contractions of the ventricles appear to take place with greater power and completeness, as denoted by increased fulness and force of the pulse.

The fifth chapter is devoted to the discussion of congenital defects, in which we are glad to find that the researches of Dr. Peacock are amply referred to. The following chapter is taken up with a description of certain affections incidental to organic diseases of the heart, as fibrinous coagula, polypi, enlargement of the thyroid body, and angina pectoris. The frequently reiterated assertion that this last complication is owing to simple weakness of the heart, is rebutted by Dr. Flint, and with good reason; but we believe that distention of the cardiac cavities plays a very essential role in this disorder, doubtless sometimes as the effect of neurotic aberration, but more frequently as the cause of this and all its other terrible concomitants. Useful instruction may be gathered from inquiry as to the common immediate antecedents of the attack, these being powerful emotional excitation, rapid walking, especially up an acclivity or in face of the wind, violent muscular exercise, or a recent heavy meal. These all tend to show that the heart is temporarily over-burthened, and, as there is already organic mischief, this is equivalent to over-distention. All men are agreed upon the necessity of stimulant pediluvia, powerful derivative measures, energetic stimulant and antispasmodic remedies; but the employment of blood-letting, either locally or generally, is for the most part condemned. But if the opinion above expressed be correct, this must be an important part of the treatment. Some twenty-five years ago, a medical friend of ours was suddenly attacked with the most urgent phenomena of angina while excited by a game at whist, which no persuasion could ever induce him to play afterwards; he was instantly bled from the arm by one of his pupils, and he always considered his life was saved by this measure; he lived for nearly twenty years after the seizure. It should be remembered, that if a sudden diminution of the quantity of circulating fluid is apt to be at-

tended with syncope, the same state may be induced by the reverse condition, to counteract which rapidly and effectively, we must have resort to the detraction of blood.

The seventh chapter, the longest in the book, is a complete essay on Pericarditis; perhaps as good as can be placed in the hands of the student. Towards its conclusion, Dr. Flint discusses the effects produced by pericardial adhesions upon the heart and circulation, and displays a fair acquaintance with the researches recently published upon this question; though, on the whole, laying more stress upon the statistics of Dr. Henry Kennedy than we think they merit, considering that they were drawn, not from personal observation, but from a variety of records, of very diverse values in such a sum. We need scarcely mention how great a change has come to pass since Hope taught the invariable bad effects, in the way of the production of hypertrophy, ensuing from pericardial adhesions. In the work before us, our knowledge at the present time is fairly summed up as follows:—

"While it is not to be denied that pericardial adhesions do contribute to enlargement of the heart in a certain proportion of cases, nor that they may also, in rare instances, tend to an opposite result, they exist not unfrequently for a long period without being followed by any appreciable morbid condition; and their tendency to the production of either organic change or functional disturbance is vastly less than was supposed by Hope."

The author considers the following to be the physical signs denoting pericardial adhesions: the area of pericardial dulness on percussion remaining unaltered in different positions of the body, and not affected by a deep inspiration; the limits of the respiratory murmur not affected by a deep inspiration; the apex-beat often suppressed, and, if not suppressed, often raised above its normal position; the apex-beat, if felt, unaffected or affected but slightly, by changes of posture from the back to either side, and by forcible inspiration or expiration; retraction of one or more intercostal spaces, together with the ribs in some cases, and depression of the epigastrium synchronously with the ventricular systole.

Unhappily, the utility of these diagnostic marks is greatly diminished by the consideration that they are rarely, if ever, marked, unless the pericardium be attached to the thoracic walls by pleuritic adhesions, and hence they become rather the indices of these latter than of proper pericardial adhesion.

The following chapters are taken up with the discussion of endocarditis, myocarditis, functional disorders, and, finally,

aneurism of the aorta; and are fully equal to the other parts of the work. After so long a review, it is hardly necessary to express, in categorical terms, our sense of the value of Dr. Flint's book. Its style is clear, and without the slightest attempt at fine writing, which too many of the author's countrymen are fairly to be charged with. In his efforts to be clear, indeed, there are many illustrations of the over-use of words, which might have been well left out. We commend to his notice the fact that the best English writers invariably place the preposition "to" after the verb "belong," the contrary being esteemed a provincialism in these countries. We take exception to the word "diagnostician;" it is ugly and superfluous. He is not satisfied with the simple phrase—physician! But these are very trivial spots, and in no wise prevent us from declaring our most hearty approval of the author's ability, industry, and conscientiousness.

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*Notes on the Dispatch of Troops by Sea.* By CHARLES J. KIRWAN, L. R. C. S. I., Assistant-Surgeon H. M. 13th, Prince Albert's Light Infantry. London: W. Thacker and Co. 8vo, pp. 81.

THESE Notes, the author tells us, are founded upon the experience of nine voyages with troops at sea, and his object in publishing them is to direct the tide of inquiry to a subject *most intimately* connected with the health and comfort of our soldiers. There is hardly an officer or man in our artillery or infantry regiments who, in the turn of duty, does not become, upon two occasions, at least, the occupant of a troop-ship. Few altogether escape foreign service; the rest are, necessarily, in their passages to and fro, subjected to all the annoyances and miseries of the sea. That these can be lessened (if not altogether got rid of), is a fact; but it is only to be done by the systematic adoption of rules and regulations, and the introduction of arrangements, both prior to and after embarkation, superior to those generally in use. It seems to have been, as if by common assent, yielded that a sea voyage was to be accompanied by inconveniences, and that these must be endured as they best may. Now, nothing can be more fallacious in principle, or injurious in practice. They can be obviated. Over the elements, man, of course, can have no control, but beyond this he has everything in his power, and there is no reason why the discomforts hitherto endured should be continued. Mr. Kirwan has ably pointed out in the work before us how this is

to be effected. The way is therein made plain, and it now remains only for the authorities to give their order that such suggestions shall in all cases be implicitly followed, and the matter is effected.

We are frequently called upon to review large tomes and ponderous volumes. We open them and peruse page after page before we arrive at a single fact worth noting, or really important. We have here, on the contrary, in this little pamphlet, an amount of valuable suggestions and practical information such as is but rarely submitted to our notice. Mr. Kirwan has done good service in publishing his views and experience. To the medical officer placed for the first time in charge of troops on board ship, this essay will be of the very greatest use, whilst the soldier must be correspondingly benefited by the carrying out of the various rules and suggestions laid down.

Mr. Kirwan's observations will, we feel assured, meet at the hands of the present Director-General of the Army Medical Department, himself a thoroughly practical man, a good reception, and we trust that it will be as cordially welcomed by the military authorities. For our own part we can only say that we consider it worthy of being printed at the public expense, and circulated throughout the army, navy, and transport service.

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*Traité des Maladies des Voies Urinaires.* Par le Docteur Ch. Phillips. Paris: Germer Baillière. 1860. 8vo, pp. 676.

The subject of urinary diseases still continues to feed the press with a lavish hand. The ardour of writers on this branch of medical literature seems in no respect to have diminished, and of the fact that this zeal is not confined to Great Britain, we have sufficiently *voluminous* evidence in the large work before us. But the supply has become more than equal to the demand, for, while the energy of the writer is unabated, that of the reader has begun to flag. The number of works, many of them far from being distinguished for their profitableness, in at least a practical sense, has amounted to an array so formidable as to defy being scanned by the most studious reader, and there is but little doubt that the shelf supports more volumes than have ever met the eye of ordinary practitioners. The work which bears the above title is certainly a valuable addition to the surgery of diseases of the urinary organs, being compiled with much care and industry, and displaying, as it does, a good

acquaintance with what has been written on the subject. But, like most other books emanating from French authors, the allusions made and the justice done to British authors are *comparatively* scanty and measured, as if by casting a shadow on the latter, the lustre belonging to the former becomes the more brilliant. Literary injustice, as well as every other form of wrong to individual deserts, may be done nearly as much by the suppression as by the open denial of what is fairly due. The French surgeons have unquestionably distinguished themselves in the department of surgery to which the present volume is devoted, by their writings, their practice, and their mechanical inventions; and amongst the number, M. Civiale, at the present day, stands pre-eminently forward in each of these respects. But have British surgeons less claim to laurels for achievements in the same field? Let the writings of Hunter, Home, and Brodie; the operations of Syme and others; as well as the mechanical inventions of Stafford, of Arnott, and a host of British surgeons, answer. If we limit attention to this part of Great Britain, are we wholly disentitled to any share of credit for writings on and inventive genius displayed in this special department of surgery? With respect to mechanical contrivances alone, Mr. L'Estrange, of this city, has done more for the important operation of lithotrity than almost any other surgeon, British or Continental. By his invention of the screw as a crushing force, he made the first step towards the simplifications and safety of lithotrity, and thus substituted an easy and *comparatively* painless means of reducing calculi for the coarse, and difficult, and horribly-torturing method so long employed and advocated by Heurteloup. Yet, though this was but one of the improvements effected in lithotritic instruments by Mr. L'Estrange, le Docteur Phillips has not made even a passing allusion to what he has done. Is this omission the result of ignorance? If so, no excuse can be pleaded, for a compiler should, above all things, take pains to give the *true* history of surgical inventions. It has been too much the practice for authors to rob the merit due to others, either by arrogating it to themselves, or by bestowing it on favoured persons; but it is the reviewer's privilege and duty to expose every form of literary theft, and to restore the stolen property to the rightful owner. This is not the place to enter into a detailed account of Mr. L'Estrange's inventions regarding instruments for lithotrity: suffice it to say, that his claim to priority in respect to the screw, as well as the merit due to him for many other improvements and modifications in these instruments, can be proved with ease and accuracy.

We have not space to do anything like justice to the large work of Dr. Phillips—the great mass of matter contained within its pages rendering it impossible to give it a full and satisfactory review. While all the parts will more or less repay the reader, we more especially recommend a perusal of the chapter which enters into a detailed account of the various forms of urethrotomy, and of the different kinds of cutting instruments for dividing strictures within the urethra. We have always energetically condemned the practice of cutting strictures within the urethral canal as dangerous, uncertain in its results, and of no permanent avail; and we think that a careful examination of all the instruments employed for the purpose ought to prove to any unprejudiced mind the truth of what is here stated. Nevertheless, it is interesting and instructive to learn the history of these instruments, and to trace the progress of the ingenuity displayed in their construction and adaptability. Indeed, the whole subject of the treatment of stricture is well and fully discussed by the author, and is deserving of the student's attention; we cannot, however, say as much for other subjects connected with the disease, many of which are dealt with in a cursory and unsatisfactory manner. The important subject of infiltration of urine, for example, receives but meagre notice, and, as to its treatment, there is hardly any description of it given. In alluding to the origin of this event, the author says:—

"Infiltration of urine is sometimes preceded by the formation of a tumour in the perineum, which remains stationary for many days. The patient who suffers from stricture of the urethra makes vain efforts to pass water; during these efforts the tumour bursts suddenly, and the urine escapes into the neighbouring structures. This urinous tumour is burst by the prolonged efforts which the patient makes to urinate, or by an additional quantity of urine driven forcibly in this direction, or, finally, by sphacelus of its walls."

The next chapter treats of urinary abscess, and is not much fuller in practical instruction:—

"When the infiltration of the urine is not very rapid, the tissues into which it is infiltrated inflame; one or more tumours of different volume are formed, very hard, and develop themselves in the perineum, in the scrotum, or at the root of the penis; more rarely they appear in the walls of the abdomen, in the thighs, in the groins, in the sacrum, &c. These tumours are sometimes very slow in their progress; often they remain stationary, and suddenly, without any appreciable cause, they give rise to accidents similar to those produced by sudden infiltration of urine."

The second part of the work embraces the entire subject of prostatic diseases. The account given of hypertrophy of the prostate gland, or, as it is most commonly called in this country, "the chronic enlargement" of the organ, is worthy of perusal. The following are the author's observations on hypertrophy of the middle lobe:—

"*Hypertrophy of the Middle Lobe.*—The middle lobe is not generally regarded as a more frequent point of origin of hypertrophy than the lateral lobes, but it is the portion of the prostate which acquires the greatest volume. It is developed almost invariably immediately behind the urethro-vesical orifice, and it never forms a tumour in the prostatic portion of the urethra; the resistance of the lateral lobes being an obstacle to its development into the canal, it extends into the bladder, where it finds a free space.

"This rounded tumour, sometimes uneven on the surface, varies in size from that of a nut to that of an egg. Its volume is considerable, especially when the entire gland is hypertrophied. Most ordinarily single it is isolated from the walls of the bladder by two furrows, which pass obliquely towards the urethra. The posterior face forms an inclined plane towards the trigone, the summit of which it raises, and with which it is merged; the anterior face, gradually contracting, terminates in a point in the urethra on the verumontanum.

"The antero-posterior diameter is ordinarily less than the transverse, and the tuberculated and uneven surface renders the presence of granulations recognisable. Sometimes the surface is furrowed with rays, which spread from the trigone and reunite in the urethro-vesical orifice; their existence is attributed to the presence of muscular fasciculi, and they divide these tumours into lobules.

"M Jarjavay states that the granulations of hypertrophied glands grow from without inwards, and, coming in contact, present the appearance of distinct masses. By an antero-posterior incision we can separate these lobules, which, separated by a fibrous lamina, seem to constitute a single lobe by their contact. M. Cruveilhier was the first to point out this arrangement.

"As the two lobes increase, the glandular substance covers the raphé which unites them, and it is not always easy to recognise the true nature of these tumours. M. Jarjavay, who has dissected a great number of them, says that nothing resembles more glandular substance than the tumours which are often developed in the intra-glandular muscular fasciculi, and which are nothing else but fibrous tumours, analogous in all respects

to the tumours of the same nature which have their origin and grow in the muscular fibres of the uterus In those cases, in place of being hypertrophied, the prostate is atrophied, and the precise seat of these growths is in the muscular walls which are interposed between the lobules. M. Velpeau, who first propounded this opinion, bases it upon the analogy of structure which exists between the prostate and the uterus. These two organs have an almost similar development in the ovum, and undergo the same lesions in old age; the basic tissue of the organs is similar, and their organic muscular tissue is not arranged in layers.

"We may add that the labours of modern anatomists tend to prove that the prostatic sac is analogous to the uterus.

"Excrescences are sometimes seen attached by broad bases or by small pedicles to these tumours, and similar to those which are found on the anterior part of the trigone.

"The colour of these tumours is at first the same as that of the surrounding parts. When they are inflamed, they become brownish and violet-coloured. They are rarely seen at their commencement, as they do not cause sufficient functional derangement then to attract attention. I have examined a certain number of these cases in my course of lectures on lithotrity, and I have seen small granulations, more or less numerous, behind the urethro-vesical orifice, sometimes isolated, at other times united together, presenting the appearance of an uneven rounded tumour, or of a transverse pad. It then becomes really a third lobe of the prostate, distinct from the two lateral lobes; it may become very large without giving rise to any external tumour, the entire volume of the gland not being augmented."

The treatment of retention of urine, arising from enlargement of the middle lobe, is the most important point in the consideration of the entire subject of prostatic disease, and Dr. Phillips gives some information upon it worthy of study.

The third part of the work treats of calculous affections and of lithotrity. With respect to the description given of lithotrity, we have to find the fault already alluded to, viz., the author's omission of any reference to Mr. L'Estrange's invention, but, with this exception, we feel that we can express approval of it in general terms. The student will find much valuable instruction upon this important operation. In speaking of the treatment preparatory to lithotrity, the author recommends the daily introduction of bougies, on the ground that this measure disposes "les organes à supporter les manœuvres et à laisser sortir les débris de la pierre sans occasionner de grandes douleurs." We think that any kind of *mechanical*

measures, as preparatory treatment, is rarely called for, and is often injurious by exciting urethral or vesical irritation, which, when the lithotritic instruments are employed, and the stone crushed, cannot be subdued, but are sure to be aggravated. Except where the urethra is unusually narrow, either congenitally or as the result of adventitious deposit, or that it is necessary to employ instruments of very large size, we see no reason for any preliminary interference with the urethral canal, and prefer leaving the part in as quiescent a state as possible until the period for the operation arrives.

We shall conclude our notice of Dr. Phillips' work by quoting his short account of lithotrity in children, an operation not often resorted to in this country:—

"It was at first supposed that lithotrity was not applicable to children, but M. Civiale has proved by a great number of facts that it can be performed even at the earliest ages, and by different processes. It is more difficult than in the adult, and demands particular care, the greater, the younger the child is.

"The intractableness of those little patients, and their excited movements, render the necessary manœuvres dangerous; it is sufficient, however, to have them held by assistants, and not tied, as some surgeons recommend. M. Civiale has the thighs of the patient held by two assistants, and a third stands near the bed to prevent the child from jumping up, which it is necessary to prevent with the greatest care.

"What would prevent the surgeon from practising lithotrity in the child is the size and hardness of the stone. As a small instrument only can be used, hard stones cannot be broken; if, on the contrary, they are not hard, but are of a large size, they require so many sittings that a favourable termination could not be expected.

"The neck of the bladder is very dilatable in an infant, and it allows very large fragments to pass into the urethra; these may accumulate in a portion of this canal, and give rise to dangerous consequences.

"It is often necessary to divide the meatus of the urethra before proceeding to break the stone, and this precaution prevents dangerous complications.

"The urine passed by these little patients after lithotrity is rarely bloody, because there is no necessity for violence during the operation."

*On the Injurious Effects of Mercury in the Treatment of Diseases.* By S. O. Habershon, M. D., London. London: Churchill. 1860. 8vo. Pp. 82.

In Dr. Habershon's brochure we have, once more, a lance broken with that unfortunate divinity, Mercury. In the whole range of heathen mythology we do not find one other name that has been subjected to such vicissitudes of human opinion: at one time lauded up to the skies, as the universal panacea for every ill to which the human flesh is heir; at another, having every mischance, every species of charge, laid at its door, and being held accountable for every unfortunate result that may occur in the course of disease; and we must express ourselves as being of opinion, that more good than harm has resulted to the human race from the doubts that have been thrown on the efficacy of mercury in the treatment of disease. Any one who cares to read the records of the past must blush to think that, in the history of our profession, there ever was a period when the efficacy of the mercurial treatment was estimated by the *quarts* of salivation thereby produced, and must shudder at the amount of misery so inflicted on suffering humanity. No school has exerted itself more efficiently in this cause than that of Dublin, and with it must honourably be associated the names of Carmichael, Alley, Moriarty, and many others, who have always steadily opposed its abuse, whilst fully alive to its value when employed with due caution. To Colles also—avowed advocate of mercury as he confessed himself to be—great merit is due for the cautions which he lays down in his valuable monograph on the subject; so much so that, now-a-days, we should almost conceive it to be a work of supererogation, a repeating of a twice-told tale, again to traverse this ground, or to occupy professional attention with another work on the subject. All our works on Materia Medica contain ample and full instruction on this point, and every lecturer considers it only a matter of duty to impress these considerations on the minds of his audience. However, Dr. Habershon seems to entertain a different opinion, and hence the present publication.

On reading this little work, the first thing that struck our mind was, that the title given to it was a misnomer. If it be simply to describe the injurious effects produced by the administration of mercury, as its name would infer, we, in the discharge of our duty, must declare that it has fallen far short of its object: some of those injurious results are altogether omitted, whilst others, that are mentioned, are described far less graphically and forcibly than in many other standard works that we

could name. We rather incline to believe, however, that Dr. Habershon's true object in giving us this brochure is to establish a fact already well known to most practical physicians, that many of those diseases in which hitherto mercury has been considered, and with justice, our sheet-anchor, *may* get well without its administration at all, and under a very opposite plan of treatment. With this view he gives us some cases in which other practitioners would have employed mercury, but in which he dispensed with it, trusting to other means of treatment, principally of an alkaline nature. Now, whilst fully agreeing with him as to the propriety of avoiding Scylla, yet we consider, in doing so, it is not at all essential to fall on Charybdis, nor is it consistent with a sound judgment to rush from one extreme into the other. In many of the cases in which he considers it worse than useless, we have had most convincing evidence that the life of the patient was, under Providence, only to be attributed to its judicious administration. In common with many of the most distinguished authorities in our profession, we have had to thank mercury for the relief it has afforded our patients in many a dangerous conjuncture. Many cases of bronchitis recur at this moment to our minds when, after every other variety of treatment had failed, on the mouth being slightly touched by mercury, the severity of the symptoms abated, and a speedy and satisfactory convalescence was established. This, however, is one of the many diseases in which the employment of mercury is said by Dr. Habershon to be injurious.

Dr. Habershon also doubts its efficacy in the cardiac complications of rheumatism, and gives us the particulars of some successful cases in which recourse was *not* had to this mineral. Of the accuracy of his statistics we entertain no doubt, though we do of the value which he attaches to them. That some cases may get well under any plan of treatment is not to be questioned, no more than that, if left to nature's unassisted efforts, similar results might ensue. What we contend for is a far more extended series of observations, before we should attempt to lay down as an axiom that mercury is not required in such cases; and even this, our readers will please to observe, is stopping far short of what Dr. Habershon contends for—that it is absolutely injurious. That we may occasionally be able to dispense with its use may be granted, but that, in such cases, it not only is not demanded, but that its use is positively noxious, is more than we can accept. Its value has been so frequently proved by the highest authorities in medicine, that we dare not doubt it; but to go farther, and say that it is ab-

solutely injurious, is to cast a slur on the acumen of our ablest observers and writers. Can our readers believe that such brilliant intellects as Hope, Stokes, Latham, and others, have had their powers of observation so blunted as to recommend the use of a medicine not only useless, but even injurious. That they might err so far as to attribute their patients' recovery to other than the right cause, though improbable, is not impossible; but that they could make so grievous a blunder as to fancy that their plan of treatment was useful, whilst, in truth, baneful, is more than, we think, Dr. Habershon will succeed in proving to us. A man may fall down and break his leg, and, perchance, recover with a useful, straight limb, on whom no splints have been employed; but will that be advanced as an argument against their value, still more to sustain a charge against them of being injurious? That the *vis medicatrix* of Nature is wonderful, is not to be questioned, but that would be but a sorry argument for leaving her unassisted.

Many of the other instances in which Dr. Habershon considers the employment of mercury as being injurious are precisely those in which he is supported by the general voice of his professional brethren. In some of those, however, he might have shown a more extensive acquaintance with the literature of his subject, by giving us the opinions of other writers who have supported a contrary opinion. We here may illustrate this observation by referring to the opinions of such able writers as Colles, Porter, and de Méric, who advocate its cautious employment even in subjects of strumous diathesis. On the whole, with regret, we must say that we cannot accept Dr. Habershon's present communication as being at all of an exhaustive nature on the subject of which it professes to treat; and, at the present moment, we fear that there is more danger of our running into the *non*-mercurial extreme, than of abusing, by over-use, this most valuable medicine.

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*Contributions to Midwifery and Diseases of Women and Children; with a Report on the Progress of Obstetrics, and Uterine and Infantile Pathology, in* 1858. By E. Noeggerath, M. D., and A. Jacobi, M. D. New York: Baillière, Brothers. 1859. 8vo, pp. 466.

In the preface of the work before us the authors complain of the hasty, off-hand, and, consequently, imperfect manner in which midwifery and the pathology of uterine and infantile diseases have hitherto been dealt with in those works which pro-

fess to give a retrospect of the progress of medicine in general, but which have appeared to them to give an unfair preference to the general pathology of so-called internal and surgical maladies, and to omit much that is important in the domain of obstetrics.

"The necessity of a book intended to supply this want will be readily understood by every one who feels a desire to make himself acquainted with the progress of science, but more particularly to those of our brethren who feel called upon to write articles of their own. A perusal of our periodicals reveals a frightful state of ignorance as to what has come before, and thus a vast amount of labour is wasted by the publication of so-called new facts and theories, which might be more usefully employed, if the books already there were more generally known. This was, hitherto, a very difficult task, because we were in want of a book which should contain the essence of all that is dispersed in hundreds of publications written in a great variety of languages. In preparing a work of this kind, we have endeavoured, at least, to give *an account of every original article or monogram that appeared to be of any importance*, while we have tried also to mention, at least, *the headings of those of less value, or beyond our reach.*"

In carrying out the objects thus proposed the authors have, doubtless, spared no trouble in selecting from a very large mass of writings the essential points of interest or importance possessed by the various treatises, the originals in most instances being not merely translated or quoted, but rather subjected to a careful analysis, and their principal features epitomized in language at once forcible and concise; in this way affording their readers, in the course of some 350 pages, a compact digest of an enormous mass of scattered writings. By no means the least important event of the year was the spirited discussion which took place in the Paris Academy of Medicine, upon puerperal fever. The points offered by Dr. Guérard, by whom the discussion was developed, are comprised under three heads, namely: the nature of the disease; its mode of propagation; and the treatment. The condensed report of this discussion, which is full of interest, is followed by an account of a rather prolonged discussion on the same subject at the New York Academy of Medicine, in which, although destitute of that brilliancy which is peculiar to French orations, and void of the vast amount of experience collected for decennial periods in French hospitals, there may yet be recognised a sound practical tendency in the remarks of the several contributors, which is of far greater value to one who reads for the purpose of acquiring information.

At page 280 we find two interesting cases, in which the plan of treatment for prolapsed funis, first proposed by Dr. Thomas of New York, was put in practice with success. Dr. Thomas attributed the great persistence of this accident, whatever may have been its original cause, to the following conditions: first, the slippery nature of the funis; and second, the inclined direction of the uterine axis. These conditions he thought might not only be overcome, but might themselves be rendered serviceable in effecting reduction, by inverting the uterine axis, by placing the woman on her hands and knees, in the posture employed by surgeons in operating on the uterus and vagina. In both the reported cases, the patient was allowed to choose her position, as soon as the head had fully descended so as to occupy the pelvic cavity. The rules of treatment pointed out were these:—1. If the cord is detected before the waters have broken, let no manual assistance be offered, but place the woman at once in position, and trust to this for its return to the uterus. 2. Should the waters have flowed away and left the cord below the head, place the woman in position, and push it up with the hand if practicable, or with a porte-cordon, consisting of a gum-elastic catheter, with a tape passed through it; if not so—3. Let no manipulation be commenced until the woman be placed in position.

In the report on the progress of infantile pathology, which forms the concluding part of the work, will be found some valuable matter, interspersed occasionally with passages of somewhat questionable propriety as regards the choice of terms. Let two examples suffice:—

"Dr. Brinton attended a boy of eleven years, whose bowels had not been moved for three months, and whose abdomen was forty-three inches in circumference. The rectum was *scraped* out, and enemata of croton oil, caster [*sic*] oil, turpentine, and gruel were administered daily, until the abdominal measure came down to the natural circumference of twenty-eight inches."

Again:—

"Mr. Russ reports shortly, indeed very shortly, a case of probable acute spinal meningitis, in a boy of three years of age. The doctor administered, every three hours, one grain of calomel, and two grains of Dover's powder. *The child did not stand it.*"

The defective orthography which occurs in many places, arising apparently from careless editing, will not fail to offend a sensitive eye, for example: 'menstration,' 'breach presentations,' 'opisthotanus,' &c. These, however, are but small matters. Not

so the next that we have to notice, and which occurs in the account of the excellent work of Drs Sinclair and Johnston, who are represented as having been assistant-physicians to the Dublin Lying-in Hospital, during the mastership of 'Dr. Skeleton'!

The report is preceded by a number of original articles, the first four being contributed by Dr. Noeggerath, and the remaining three by Dr. Jacobi. In the first of these papers are detailed four cases in which the induction of premature labour was effected by the method of Cohen. The mothers recovered in all the cases, but two of the infants were lost; these two, however, were cross-presentations, requiring the performance of version. Such a limited number of observations is, of course, insufficient for a final discussion with regard to the method; but, added to the statistics already known, they may tend to throw additional light upon the operation, and serve to determine its true position. The procedure here alluded to, although introduced into practice by Dr. Cohen, of Hamburg, about fourteen years ago, appears to have been originally conceived by Dr. Schweighaüser, of Strasbourg, in the year 1825: it consists in the injection (with some degree of force) of a quantity of warm water into the uterus by means of a catheter introduced within the os, and directed upwards between the anterior wall of the uterus and the fœtal membranes. The author expects that this measure will supplant altogether the employment of the douche.

The remaining contributions consist of papers on injection of caustic solutions into the uterine cavity; employment of pessaries, with description of a new instrument; pathogenesis of uterine polypi; invagination of the descending colon in an infant, with repeated hemorrhages in the transverse colon; oxysulphuret of antimony as an expectorant in inflammatory diseases of the infantile respiratory organs; etiological and prognostic importance of the premature closure of the fontanelles and sutures of infantile crania.

We must now bring to a close this brief notice of an elaborate and useful work, and in doing so we must not omit to observe that in all books of this kind a well-arranged alphabetical index cannot be conveniently dispensed with; we trust, therefore, that our transatlantic fellow-labourers will accept a friendly hint, and, in their next annual retrospect, endeavour to supply this important desideratum.

> "Absente auxilio, perquirimus undique frustrà;
> Sed nobis *ingens* Indicis *auxilium.*"

*Essays in Medical Science.*—I. *On the Nature of Inflammation* II. *On the Encephalic Circulation, and its Relation to the Physiology of the Brain.* By James Cappie, M.D. Edinburgh: Sutherland and Knox. 1859. 8vo, pp. 103.

Dr. Cappie is an elegant and evidently well practised writer. Of the importance of the subjects considered in these Essays there can be no question. The consideration of the nature of inflammation has occupied the attention, and formed a great part of the labours, of some of the greatest intellects that ever studied medical, or perhaps any other science; and right and fit is it that it should be so, for on a true apprehension of it hang almost all our theories as to the nature of diseased action.

The successive advances we have attained in physiology have brought us to new and more complete views of this subject. Theories, more or less crude, have gradually been eliminated, and we have alike got rid of the earlier humoral pathology, with its *error loci*, and of the theories attributing inflammation to spasm, or to an increased or diminished action of the blood-vessels.

Physiologists have now come to recognise the molecular life of the tissues, to see that all the structures of the body are alive, and contribute to the life of the whole, instead of being themselves dependent on nerves or bloodvessels for their life; and that, for the maintenance of their life, and the manifestation of their properties, it is necessary that they should undergo certain processes of waste and repair, the means of which are supplied by the blood, the processes being under the control of the nervous system, but essentially independent of it, and performed by the tissues themselves, in virtue of their own life. These processes of waste and repair are inseparably connected, and are by writers grouped together as forming the function of nutrition. Pathologists are now unanimous in attributing inflammation to a perversion or abnormal action of the processes of nutrition. It is the nature of this perverted action that Dr. Cappie discusses in the first of the two Essays of which his volume consists.

The question at issue is, as to whether this perversion consists in an increased or diminished action of the processes of nutrition. Taking Alison as his leader, but going much further than we think this great pathologist would himself have done, and than his words seem to us to justify, our author argues, in

opposition to Williams, Bennett, Carpenter, Paget, and Wharton Jones, whose remarks, he says, "have rather tended to make the theory more obscure than ever," that the perversion consists in an increased action of the nutritive changes.

In support of this view he argues, first, that there is an increased circulation in an inflamed part, and "he is not prepared to admit that stagnation occurs in the active stage of the process at all." We feel that we may at once withdraw from the argument at this point, for we regard stagnation as an essential element of inflammation; excited vascular action, without stagnation, we regard as simply hyperemia. In this state the nutritive processes are not perverted, and inflammation begins only when these processes are perverted or suspended, when, as a consequence, the blood becomes stagnant. This we believe to be fully established by many and repeated microscopic observations, and Dr. Cappie has not disproved it.

The second clause of the argument is based on the increased quantity of fibrin found in the blood of patients labouring under inflammation. The new "German heresy," as one of our contemporaries called it, that fibrin is an excrementitious substance, is adopted, but as no new proof of the truth of this theory is brought forward, we do not think it necessary in our present limited space to pursue the matter further, but beg to refer those who wish to investigate it to an able article in the "British and Foreign Medico-Chirurgical Review" for April, 1851.

Notwithstanding all Dr. Cappie's arguments in favour of a contrary view, we still adhere to the doctrine that in inflammation there is diminished or even suspended action of the nutritive processes, as evidenced by the atrophy, softening, degeneration, or even total death of the inflamed tissues resulting from it.

Though Dr. Cappie attributes inflammation to increased action, he is not an advocate for much depletion. On this we must allow him to speak for himself:—

"Assuming that our object is to arrest the progress of inflammation by modifying the supply of blood to the part affected, the question arises, How is this to be accomplished most effectually, and with least expense to the vital resources of the system? Shall we diminish the entire volume of blood, or lower the action of the heart, or drive the blood to other parts? To assist us in resolving this question, an important point in the general pathology of inflammation requires to be borne in mind.

"'There are none,' says Dr. Alison, 'who resist the exciting causes of inflammation so well as those in whom the blood is abun-

dant and the vascular system vigorous. But the tendency to inflammation is remarkably given by all permanent causes of debility, by imperfect nourishment, impure air, long-continued heat or cold, excessive evacuations, and intemperance.'

"At first sight it might be supposed that these statements bear directly against our own theory of inflammation. We have regarded the process, in its acute stage, as essentially one of excitement; and how is it, therefore, that it is not most readily produced when 'the blood is abundant and the vascular system vigorous?' A closer consideration of the subject will prove that our views are in entire harmony with the conditions that regulate the local distribution of blood. The 'permanent causes of debility' diminish the power of nutrition to preserve the equilibrium of the circulation, and, therefore, favour the more free determination of blood to the seat of inflammatory action.

"A constant antagonism is exerted between all parts of the body in relation to the circulation. If A and B are supplied with blood from one vessel, the amount which A will receive will not altogether depend upon the amount of stimulus applied to it, but also on the intensity of the attractive force operating at B, and on the facility, therefore, with which the latter will permit itself to be deprived of a portion of its supply. And so, in the general system, it is not only the amount of stimulus applied to an organ or texture which determines the amount of blood it shall receive, and, consequently, the effect that will be produced on nutrition. The result will greatly depend on the degree of force required to derive that fluid from other parts of the body. The readiness with which the latter may be deprived of their natural proportion of blood, will have an important influence in aggravating or repressing the tendency to local excitement,—in favouring or counteracting the influence of the stimulus. If the operations of nutrition in the system generally be vigorous, the textures will with difficulty be deprived of their natural supply of nutritive fluid; but if nutrition be languid, and the attraction for blood be in consequence feeble, the latter fluid will, with comparative facility, be determined in greater quantity towards any point at which operations causing a stronger attraction for it have been excited. This must be at least one of the reasons why, in enfeebled constitutions, inflammation takes place with greater certainty on exposure to its exciting causes.

"In the treatment of inflammation, then, the object must be to restore at the same time the balance of the circulation and that of nutrition. And we can readily suppose that, in favourable cases, blood-letting, by moderating the supply of plastic materials, may often have a very powerful influence in 'disposing to a favourable termination,' or even in 'cutting short' the morbid action. It is also obvious, however, that it cannot generally be trusted to for restoring the balance of nutrition. The loss of blood will tend to exert a depressing influence on this process throughout the system

generally, as well as at the seat of the disease. It may also happen, that at the latter part the operations are still *relatively* more active than at other points, and thus inflammation may go on unchecked, or it may even be aggravated.

"In the majority of instances, then, our object will be more efficiently accomplished by stimulating nutrition in other parts of the system, and so causing an increased amount of blood to be determined towards them. Not the least important means of doing so is to act on the various organs of excretion. A beneficial effect is thus produced in two ways; 1st, by directly affecting the balance of the circulation, and deriving the blood from the seat of increased action towards other parts, for example, the mucous surface of the intestines; and, 2nd, by eliminating impurities from the blood, and so removing one source of depressed nutritive force in the system generally.

"In some cases tonics may be employed to modify the distribution of blood. Their action, in this respect, is remarkably illustrated by the action of quinia in the cure of intermittent fever. In the latter disease the diagnostic phenomena are associated with, if not occasioned by, general disturbance in the balance of the circulation at regular intervals; and this, again, is connected with a weakened state of nutrition in the system. At the commencement of a fit of ague, a less amount of blood than usual is determined to the surface and extremities of the body, while congestion takes place in the internal viscera. Now, as there is no obvious physical or biological cause for the occurrence of this phenomenon, and we are consequently ignorant of any action to subdue or counteract, the rational mode of treatment must be to adopt such means as will tend to preserve the equilibrium of the circulation. If, by the introduction into the blood of any substances having an irritating or stimulating effect on nutrition, we can cause that process to be carried on more vigorously, then, on the one hand, the attraction for nutritive fluid is rendered stronger, and the textures are deprived of their supply of blood with less facility; and, on the other, the tendency to passive congestion is counteracted. If, in this way, the simple equilibrium of the circulation be preserved, the characteristic phenomena of the fit of ague will be prevented from manifesting themselves. And such we believe to be the *modus operandi* of quinia and arsenic in its cure. They act on nutrition generally, and cause its operations to take place with increased vigour. Their influence is exerted, in the first instance, on the ultimate structures of the texture, rather than directly on the blood-vessels. In most cases of acute inflammation, however, the system is not in the condition most favourable for the administration of tonics, and the medicines, therefore, that have a derivant or eliminant effect will be most likely to prove beneficial."

The essay on the encephalic circulation is an ingenious attempt to explain some of the obscure points in the physiology

of the brain, by the application of the laws of capillary circulation, as affected by the nutritive processes. The author seems to us to be much hampered by his adherence to the views of Dr. Kellie as to the exclusion of the vessels within the head from the effects of atmospheric pressure. The subjects specially considered are, the conditions of sleep; of concentrated thought; and some of the phenomena of mesmerism.

Though not agreeing with the author in many points, we thank him for the publication of his Essays, as calculated to call attention to an important item in the physiology of nutrition and of the brain.

# PART III.

## MEDICAL MISCELLANY.

### TRANSACTIONS OF THE BELFAST MEDICAL SOCIETY.

SESSION 1859–60.

JANUARY 2, 1860.

DR. HEENEY in the Chair.

PROFESSOR GORDON read the following cases:—

On the 11th of December, 1858, I was requested to visit a woman, 70 years of age, who was supposed to have sustained a *Compound Fracture of the Metacarpal Bone of the Right Thumb.* On examination, instead of a fracture, I found the base of the first phalanx dislocated directly backwards on the metacarpal bone. The thumb was extended; above its base there was a well-marked depression, in which the extensors could be seen, and felt tense and resisting. On tracing the metacarpal bone from the trapezius downwards, it was felt to pass towards the palm of the hand, and to be on a plane anterior to the base of the first phalanx; opposite to its head there was a transverse wound, sufficiently large to admit the end of the little finger, and with which I felt easily its smooth cartilaginous surface. She says she was violently thrown down by a horse and cart, and that the injury was caused by the hand coming in contact with the street.

Extension was made in a direction forwards and upwards, and the head of the metacarpal bone, at the same time, pushed backwards. This procedure failing, the thumb was bent backwards, and the head of the metacarpal bone pushed in the same direction, but this also failed. I now introduced a tenotomy knife into the wound, and divided the lateral ligaments, and then attempted the reduction, but with no better success. As violent attempts at reduction had been made for a considerable time before I saw the patient, and besides, dreading

the serious consequences that might result from further injury to the softer textures, the wound was slightly enlarged, not more than a line, or at the utmost two lines in extent; then, bending the thumb backwards, the entire head of the metacarpal bone projected sufficiently to enable me to remove it with the cutting forceps. On extending the thumb, and pressing gently on the metacarpal bone, it slipped easily into its place, with a soft grating feel. On the 24th of October, six weeks after the accident, the wound was nearly healed; slight swelling over the joint, which was scarcely painful on moderate pressure, or on passive motion.

On December 1, 1859, nearly a year after the accident, on visiting her and comparing the hands, the injured thumb is shorter by four lines, and the motions of the joint are almost as extensive on the right or injured side as on the left. She cannot sew as well as before the accident from the shortness of the thumb; but this is the only inconvenience experienced.

Without entering into detail as to the various opinions which have been from time to time promulgated as to the causes which render the reduction of this dislocation so difficult, I shall merely state the views now held, and which I have held for more than fifteen years. The circumstances which determined me to adopt them were experiments made in the dissecting-room. Having removed all the soft parts, excepting the lateral ligaments and fibrous tissue on the palmar and dorsal surfaces of the metacarpo-phalangeal articulations, I then dislocated the head of the metacarpal bone by pushing it forwards, whilst at the same time the phalanx was bent backwards. During this movement the fibrous tissue on the palmar surface is first made tense, and ultimately violently distended; then bursting, the head of the metacarpal bone escapes and projects through it, and lacerated tissue becomes interposed between the bones. It is probable, in the living, when the force which caused the dislocation has ceased, that the flexor pollicis propius, acting on the phalanx, inclines it towards the palmar surface, thus bringing the dorsal and palmar surfaces of the metacarpal bone and first phalanx into opposition, the fibrous tissue intervening. Now when we make extension to reduce a dislocation of the first phalanx upon the metacarpal bone of the thumb, we cannot draw it directly downwards; the attachments of the lateral ligaments and heads of the flexor brevis give a direction to this force; they cause the displaced bone to describe an arc of a circle around the head of the metacarpal bone. In this movement the fibrous tissue, being interposed, is caught by the head of the metacarpal bone, becomes fixed, and prevents the base of the phalanx passing into its normal situation. In a dissection of this dislocation recorded by G. J. Adair Lawrie (Med. Gaz., vol. xxi. p. 92), he says: "The sesamoid bones were not connected with the metacarpal bone. I have no doubt they were placed between the metacarpal bone and phalanx." In the case just related, I regret that, after the removal of the head of the bone, I did not introduce the end of the little finger into the wound to determine the position

of the sesamoid bones. However, if the displacement backwards be considerable, then the sesamoid bones will be locked between the respective surfaces. Extension will be of no avail, but positively injurious, by causing contusion of the interposed bones and tissues; and if it be possible to reduce the dislocation, it will be by extending the thumb backwards, at the same time rotating it gently, and having thus removed the interposed structures, we push the head of the metacarpal bone backwards, and the base of the phalanx downwards and towards the palmar surface.

Some authors (Mr. Stanley, Med. Gaz., vol. xxxv. p. 100) suppose that the flexor tendon, being thrown on the inner side of the joint and turned on the dorsal surface of the metacarpal bone, offers great resistance to the reduction. If the first phalanx of the thumb be dislocated directly backwards, the flexor tendon cannot be interposed between the surfaces, and therefore cannot materially oppose the reduction. However, if the phalanx be dislocated obliquely inwards, so as to rest on the dorsal and ulnar surfaces of the metacarpal bone, then it might happen, and it is highly probable, that the flexor tendon has slipped in between the bones, and it will offer an amount of resistance not to be overcome until that tendon has been divided by subcutaneous incision. However, it is an anatomical fact not to be questioned, that in this dislocation the tendon of the flexor pollicis propius is displaced inwards, resting upon the inner side of the phalanx, and over the inner part of the dorsal surface of the lower end of the metacarpal bone.

*Sanguineous Cystic Tumour.*—As the sanguineous cystic tumour is a rare disease, and as the following case was effectually cured by repeated evacuation of the cysts by the trocar and canula, without any untoward event, or even without the patient discontinuing his laborious employment,—it is therefore practically interesting as assisting in establishing simple tapping as a mode of treatment, which should be fairly tried before we have recourse to more severe measures, in a tumour which could not have been extirpated with safety.

"A lad, about 16 years old, was under Mr. Stanley's care, with a large, oval, and somewhat pendulous swelling in the left side of the neck, which had existed many years, and appeared merely subcutaneous. It was punctured, and about sixteen ounces of fluid blood escaped, which soon coagulated. After this the cyst closed—a result more favourable than may generally be anticipated from such simple treatment; for usually these, like other cysts, are not obliterated unless after free incision."—Paget's *Surg. Path.*, vol. ii. p. 48.

"Sept. 18, 1847.—Peter Muldoon, 48 years of age, a labourer, consulted me on account of a tumour of the neck. On examination I found in the inferior and lateral part of the right side of the neck a tumour almost as large as a cocoa-nut, projecting over and below the anterior surface of the clavicle to the extent of two inches; its surface is botryoidal; it is soft and elastic; and although it does not fluctuate, it has the feel of being made up of several tensely filled

cysts. It is not painful on pressure or on being handled, and moves freely beneath the parts covering it, which are of no great thickness. He says that his health has been always very good, and he first perceived the tumour about fourteen years ago, when it was about the size of a bean. On pointing to the situation where he first felt it, on the opposite side of the neck, it corresponded to the outer border of the sterno-mastoid muscle and the inferior lateral triangle of the neck. At that time it was soft and free from pain. It remained almost stationary for a year; then it suddenly increased to the size of a pullet's egg. Four years from the present, it was not larger than a turkey's egg, but after that it has been gradually enlarging. It was formerly quite soft, and gave him no uneasiness, but latterly it has become harder and more painful, especially when he stoops much in making thorough drains. I passed a fine exploratory trocar into the tumour, and drew off about an ounce and a half of bloody fluid.

20th. On the morning of the 18th the trocar was introduced into the anterior inferior part of the tumor; but to-day I pierced the posterior inferior part, and in doing so experienced considerable resistance from the denseness of the walls of the cyst. Two and a half ounces of bloody fluid escaped, causing a considerable diminution in the size and tenseness of the tumour, which now feels quite flaccid, and moves very freely beneath the integument and superficial fascia, and seemingly consists of several cysts and firm nodules, united by dense fibrous bands. On examination of the fluid, twenty-four hours after its evacuation, it was found to have separated into a rusty, brownish, yellow-coloured fluid and a reddish grumous matter; when it was shaken, it presented the same appearance as when withdrawn from the tumour.

22nd. The tumour has regained its former size; a larger trocar was introduced, and a little more than four ounces of fluid was removed, part of which was of a florid hue. The external jugular vein is moderately distended, and seems to pass into the tumour. On examining the fluid four hours afterwards, it was found to have separated into a dark brown serous fluid and a loose clot, the bulk of each being nearly equal.

28th. Has been at his work, as usual, and enjoys good health, but last night the feeling of distention and pain returned; however, he went to his work this morning, but was obliged to leave off. Three ounces of fluid, as dark-coloured as treacle or tar, were removed, affording him, as usual, relief from pain and the feeling of distention in the tumour.

Oct. 4th. Has been following his usual work since the 28th of September. The tumour is not so painful nor so large as formerly; it feels more firm, is more movable, and its boundaries are better defined. Its chief attachments are behind and external to the outer border of the right sterno-mastoid muscle, which can now be traced very distinctly; the levator angular scapulæ forms its posterior, and the clavicle its lower boundary. Three ounces of fluid were removed

to-day, after which it is much smaller than on any previous occasion, and it now feels as if composed of numerous firm nodules and cysts of various sizes connected with each other.

15th. Scarcely three ounces of fluid were removed to-day, after which it was much smaller than formerly. So decided is the diminution in size, not only before, but after being tapped, that there seems every prospect of the disease being cured by the treatment adopted.

Nov. 1st. The tumour is soft, relaxed, flabby to the feel, and free from pain. The external jugular vein seems more distended, but this is probably owing to the diminution of the bulk of the tumour.

11th. The tumour has progressively diminished in size. The external jugular vein looks even more distended.

Dec. 1. The tumour is now about one-half its original size, and about one-third less than the last time I saw it. It seems to be entirely composed of solid nodules and of the walls of the cysts.

On looking over the history of this case, we shall find that the patient was under observation for a period of 73 days, and during that time the tumour was tapped six times. At each tapping, excepting the third, the blood withdrawn was of such a character, and so altered, as to leave no doubt of its having been in the cyst for some time. After the fourth evacuation the tumour began to show unquestionable signs of improvement, being then not only smaller, firmer, and more movable than previously; after that it gradually diminished in size, until at length it felt as a firm nodulated tumour.

As the term 'sanguineous cystic tumour' is of a generic rather than a specific signification, implying that the cyst contains decolorized blood, the question then arises—What was its specific character? Mr. Paget says: "Some of these cysts with blood are found in the same position and circumstances as the serous. Thus, in the neck, a series of cases of blood cysts might be collected, exactly corresponding with the serous cysts in that part, and like them, probably derived from various origins, some lying in the thyroid gland, some near it, some traceable to connexion with vascular nævi, some of proper origin."—*Surg. Path.*, vol. ii. p. 48.

The aqueous encysted tumour of the neck is a single cyst, whereas in this case there were several cysts and nodules. The tumour was too far removed from the thyroid body to render it probable that it was connected with it. It seems to me to be an instance of a vascular nævus; at least in comparing it with a tumour of this description which I removed from the scalp, and exhibited at the Clinico-Pathological Society, the resemblance is very striking.

On examination, the tumour was found to be invested by a thin, fibro-cellular capsule, oval in shape, and made up of lobes or cysts. On making an incision into it, the knife grated through it, and on passing my finger along the cut surfaces, they felt rough, and this roughness was due to a large quantity of calcareous matter imbedded in numerous small cysts. My first impression was that the tumour

was sebaceous and had undergone calcareous and malignant degeneration. But, on examining it more carefully, with the object of determining the source of the hemorrhage referable to itself, I found it bore an exact resemblance to those cases of cystic tumours developed in nævi, and which have been described by Mr. Lawrence in the twenty-second volume of the Medical Times. There are two cysts, sufficiently large to admit the point of the finger, which contained blood alone; these communicated with several others, and with a large vessel divided in the first incision. There are several other cysts, one of them of the size of a large filbert, in which there was a slightly yellow serous fluid and a quantity of fibrine; a third set, of which the main bulk of the tumour consists, are entirely filled with cholesterine, granular, or molecular matter, free or irregularly aggregated, resembling corpuscles, and a paste-like and nodulated calcareous matter.

*Extra-capsular Fracture of Cervix Femoris.*—During the session of 1858–9, I exhibited at the Clinical and Pathological Society a specimen of extra-capsular fracture of the cervix femoris, in which the amount of shortening of the limb was so trifling as not to be capable of detection by the most accurate measurement; and besides, the same specimen afforded us an example of the same fracture, in which shortening might result from two different conditions: viz., impaction of the neck, and splitting off of the shaft of the femur from the trochanters. The specimen which I now show is one of the preparations belonging to the Surgical Department of the Queen's College. The fracture is an old one, firmly consolidated, and its history is unknown. There is not only no shortening, but the angle formed by the neck with the shaft is actually increased instead of being diminished. This latter circumstance must indeed be very seldom observed, for M. Malgaigne has never seen it:—"Dans les fractures complétes, presque constamment la penetration est plus forte en bas qu'en trant, ce qui atteste que la violence extèrieure a agi de manière, a diminuer l' angle plutot qu'a l'élargir, et jamais on n'a vu cet angle agrandi d'une façon quelconque."—*Traité des Fractures*, p. 690.

I have put the fractured bone and that of the opposite side in a box, the angles of which are right angles and the ends parallel to each other, and the only perceptible difference in the length of the two bones is, that the sound bone, when the box is overturned, remains in its place, whilst the fractured one falls out. On comparing the angles formed relatively between the necks and shafts of the two bones, it is quite apparent that the angle formed between the neck and shaft of the fractured bone is considerably more obtuse than that of its fellow.

The fragments are three in number: the first consists of the head and neck; the second, of the posterior and superior part of great trochanter; and the third of the shaft, lesser, and a part of the greater trochanter. The fracture through the base of the neck is that which usually occurs: viz., posteriorly at the junction of the neck with the

posterior intertrochanteric ridge; above, internal to and beneath the digital fossa; and in front through the anterior intertrochanteric ridge; and below, immediately above the lesser trochanter. Superiorly posteriorly the neck is driven deep into the cancellated of the great trochanter, but below, immediately above the lesser trochanter, the neck, instead of penetrating, as is usually the case, started inwards and slightly downwards, resting upon, instead of penetrating, between the broken surfaces. Hence, from the penetration of the trochanter above, and the inclination of the neck inwards below, have we the angle formed between the neck and shaft increased, and the shortening resulting from the impaction above compensated by the more vertical inclination of the head and neck. The apex of the trochanter, the posterior intertrochanteric ridge, and about one inch in breadth of the trochanter, form one fragment, which is displaced slightly backwards. The neck of the femur, from the posterior penetration, forms with the shaft a salient angle in front, and hence, from this rotation, the foot was so much everted as to look directly outwards.

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*An Address to Medical Practitioners respecting the Effects on Children of Mercurial Treatment of Syphilis in their Parents.* By Dr. F. C. FAYE, Professor in the Faculty of Medicine in the University of Norway, Senior Physician to the Maternity, and to the Clinical Hospital for Sick Children at Christiania, &c.[a]

AT a time when the opinions of medical men as to the utility of a mercurial treatment in constitutional syphilis appear to be very contradictory, as has, moreover, been the case several times during the course of the last few centuries, it appears to me that it ought to be of great importance to obtain the well-established result of numerous medical observations of the state of health of children born of syphilitic parents who were treated by mercury.

We know very well that a great number of these children have been impregnated with the dyscrasia; and we also know that new modes of treatment, from which mercury is excluded, have failed in producing the wished for results in children; and especially, that the treatment by inoculation with chancrous pus (otherwise called "syphilization"), and the simple derivative method, have disappointed our expectations in this respect. It is clear that the absolute condemnation of mercurial treatment of late years pronounced by some physicians, ought to be based on something else than superficial appearances; and that, in order to be final, the comparison between the two methods ought to be carried out with precision and accuracy.

The surest and most simple proof of the cure of syphilis, the

[a] Translated for the author, from the French, by William Daniel Moore, M. B., M.R.I.A., Honorary Member of the Swedish and Norwegian Medical Societies.

true touchstone showing that the disease is destroyed, or at least so neutralized in the system that the function of the ovaries and testicles remains intact, should undoubtedly be sought for in the circumstance, that the children of parents who have been syphilitic have been born healthy, and do not subsequently become affected in anyway evidently attributable to the syphilitic dyscrasia.

The time has not yet arrived for deciding to what degree this test, important as it is both in a humane and therapeutic point of view, shall afford a satisfactory result with respect to several new non-mercurial modes of treatment; but on the other hand, we have the experience of centuries to show, in contradiction to the repeated condemnation pronounced by the anti-mercurialists, that many children produced by syphilitic parents, who have been suitably treated either by mercury alone, or with mercury assisted by other remedies, have been born without any trace of the dyscrasia, and have continued healthy, a favourable result, which is obtained more frequently when the father alone has been affected. On this point we must ever remember, that the father may, although imperfectly cured, sometimes procreate a healthy child with a woman who is perfectly sound.

To enable us to judge with certainty, and to base our opinion upon an extensive induction, it is necessary, I think, to draw not only upon hospital, but also upon private practice, to ascertain the effect of mercurial treatment upon children. I, therefore, take the liberty of requesting each practitioner who is interested in these questions, and who is in possession of well-established facts bearing on them, to be good enough to communicate the results of their experience in some mode, and to give me the references necessary to enable me to find them, if they are already published. I would also request the several medical societies and editors of journals kindly to assist in the publication of the wished-for researches. What I should desire to be briefly informed of is as follows:—How many children, born of syphilitic parents who have been treated with mercury, have come into the world healthy, and have continued so? and how many have succumbed to the dyscrasia, or have been successfully treated?

Also, how far the observation, long since made, has been found correct, as to the favourable effect on the health of the children of mercurial treatment undergone by the mothers during their pregnancy, when these mothers have previously produced syphilitic children? However, we must here bear in mind the remarkable fact, that women may purify themselves by repeated accouchements, and may finally give birth to healthy children after having had several diseased ones.

In order to attain the above object more completely, it is very desirable that the reports should state whether the father, or the mother, or both, have been affected. In this mode we may more fully test the accuracy of the generally received opinion, that fathers who have undergone treatment produce healthy children more frequently than mothers, even apparently cured.

To obtain conclusive information, it will not suffice to observe the children of the lower classes of the population who have been treated in hospital, because the members of these classes often lead an irregular life, and are exposed to relapses. Persons in easy circumstances, on the contrary, who have been treated at home, and have subsequently had several children born in wedlock, are more under the control of the physician, and we may have more confidence that the results of observation will in such instances possess a real value in enabling us to judge of the influence of antisyphilitic treatment upon children.

It is, therefore, to my brethren in all countries, and particularly to those engaged in private practice, that I have the honour of addressing my request to collect their observations, which I should wish to arrange, as soon as I shall be informed of their publication, or shall have them specially sent to me, so as to render them subservient to a comparison of the modes of treatment in different countries.

*Christiania, December* 5, 1859.

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*Account of the Illness and Death of Professor Alison.* By Dr. Newbigging.

On the 27th of May, 1846, whilst prescribing for a patient in the Royal Infirmary, and surrounded by a numerous assemblage of students, Dr. Alison suddenly fell down, attacked by a violent convulsion fit, preceded by a gentle scream or cry. The alarm spread quickly through the house, and Dr. Paterson, then one of the physicians of the Hospital, was the first to go to the succour of his friend and former teacher. That gentleman, in a recent communication to me from Tiverton, says, "that he found Dr. Alison lying on the bed of the patient for whom he had just been prescribing, his neckcloth unfastened, breathing stertorously, and quite insensible; not above a few minutes had elapsed from the time of the seizure." Dr. Paterson's impression, from the appearances presented, was, that the attack was of an apoplectic nature. There was evidence of congestion and pressure on the brain, the pulse was full and labouring, and the patient quite incapable of being roused. The report of the bystanders was, that the illness had commenced with convulsions of an epileptiform character. Still he was by no means satisfied that the case was simply of this nature. The treatment at the time could not be doubtful, as Dr. Paterson remarks, and accordingly, after removing Dr. Alison, on the bed on which he lay, into the adjoining room or closet off the ward, he was bled from the arm—the blood flowing freely. On the loss of a few ounces, Dr. Alison became conscious, and other medical officers of the Hospital, including Drs. Christison and Trail, having arrived, he had the advantage of acting in concert with these gentlemen in the management of the case. Dr. Christison informs me, that the delirium which at this time followed the attack was of a violent, and

for such a man, of a highly demonstrative character. About three hours from the commencement of the attack, Dr. Alison was able, with assistance, to walk down stairs, and return home in his carriage. He ate his dinner with appetite, felt well, and was able to drive a distance of about six miles, to his country-house. He slept quietly during the night; and next day he was, apparently, in his usual state of health.

Dr. Alison remained for some days at Woodville, happy and comfortable, reading, and enjoying the beauty of that place. He resumed, soon after, his usual duties; but I believe that, for a time, he very properly absented himself from the Hospital. The attacks, notwithstanding, recurred at intervals.

On the 1st of December of the same year, he was attacked for the fourth time, under peculiar circumstances. He had occasion to visit a patient in Roxburgh-terrace, and whilst walking, his foot slipped on the ice, and he fell on his back. He was not in the least stunned, and declared positively that his head received no blow. He rose immediately, but soon felt slight pain in his forehead. He did not however, return home, but entirely forgetting, as was his wont, the necessity of care of himself, he continued to visit his patients, one of whom resided about three miles from town. On returning from this duty, he felt unwell, and desired the coachman to drive home at once, instead of to the house to which he had at first directed him to go. After this he remembered nothing. The coachman felt a jerk in the carriage, and, looking back, saw that his master had fallen down. Finding that Dr. Alison was insensible, the man stopped at the nearest apothecary's, in search of aid, who directed him to Dr. Handyside's house, in York-place. By the time he arrived there, consciousness had returned, but, from the state of the pulse, Dr. Handyside advised his being cupped, which was done at once, Dr. Alison returning home afterwards. The after-effects of this seizure were in all respects similar to those described as having occurred on the first occasion—no uneasiness, no feeling of discomfort. At the time it was thought that this attack was possibly connected with the fall upon the ice. The late Dr. Davidson saw Dr. Alison at this time, along with his other medical friends, and, in consultation, it was resolved to try the effect of a seton in the back of the neck, which he continued until his death.

The fits recurred, as I have said, at intervals of about six weeks, until 1850, generally two in number, with slight giddiness between the attacks, and were followed by many hours of sound sleep, from which the patient awoke quite well. During 1850 they were more numerous, being eleven in number, with occasionally three fits at one illness. In 1851 there were fifteen, and these were now becoming more severe. In the following year thirteen, increasing in severity. In 1853 fourteen, two of which were followed by feelings of oppression and discomfort—sensations which, however, soon passed off. In 1854 the number of attacks were reduced to ten; but the separate epileptic fits were more numerous, much more severe, and followed by symptoms of cerebral congestion, of a formidable character.

For example, in April of that year, I find it recorded that he had six fits, followed, on return to sensibility, with headach and nervous excitement—the latter state continuing during a fortnight, and then going off entirely.

On the 23rd of October, there having been four attacks since the one in April, at 6 A.M., he had a fit, was well during the day, but at 11 at night he was again seized, and had eleven very severe attacks before the same hour next morning. These were followed for two or three hours by violent excitement, with constant motions of his arms, and great restlessness. I saw him early on that day, and found him quite insensible, almost comatose, with flushed countenance and turgid veins. His case at this time assumed a very formidable aspect, and it appeared to me that recovery was doubtful. He was cupped, and as he was able to swallow when fluid was put into his mouth, an antimonial mixture was prescribed. Dr. Christison saw my patient with me in the evening, at which time he was returning to consciousness. On my visit next morning, I found there had been no return of the spasms since 11 in the morning of the preceding day, but the mind was languid, and at times incoherent. He expressed himself as feeling very ill, and that he was paralyzed. On the seventh day of this attack he became suddenly delirious. Leeches were applied for the second time, and again on the following day, after which the delirium passed off, and he became quite calm, so that at Dr. Christison's and my visit, on the tenth day of the attack, he was again in his usual state of health, and in a week from this date he was able to walk out.

In 1855, the seizures numbered only seven, but the fits, in all, amounted to sixty, there having been, on the 4th of May, so many as eleven severe fits in twelve hours, but in the following year even this formidable seizure was exceeded, on 19th January, when, from 10 o'clock on that until 12 on the following day, he had twenty-two fits. This attack was altogether a very severe one, and resembled much the one described above, as occurring in October, 1854. In 1856, the seizures were eight, the fits eighty-six. In 1857, the seizures were seven, the fits sixty-two. In 1858, seven seizures and fifty-four fits. In 1859 the seizures were five, the fits fifty-nine.

The nature of the terrible attacks to which Dr. Alison had become so great a martyr gradually underwent certain changes. They became less numerous, but the fits were greatly increased in severity, and during this year the condition of my patient after the fits was very formidable, and most painful to witness—the delirium assuming more and more the maniacal character; and latterly the difficulty of restraint, even with the assistance of two or three male attendants, was very considerable. Although it was my custom to give my patient, during the stage of sleeplessness and nervous irritability, henbane with camphor and prussic acid, even whilst administering antimony, I had not thought it prudent to use opium, in consequence of the existence of so much cerebral congestion. With the sanction, however, of Dr. Smith, who saw Dr. Alison in a maniacal

paroxysm with me in March of this year, I ordered large doses of solution of morphia and antimony to be given, as soon as the patient indicated signs of delirium. I find it recorded that, after five doses, at six or eight hours' interval, he fell asleep, and in nine hours awoke, calm, quite himself, and from this time he steadily recovered. This occurred on the fourth day from the illness. Such was the usual effect for several future attacks.

On the 4th July he had seventeen fits, accompanied, as was the case latterly, with insensibility, during twenty-four to thirty hours, but this passing off under the use of cold to the head, leeches to the temples, antimonials, and purgatives. On the approach of the maniacal delirium, he got the large doses of opium and antimony, with its accustomed good effect, producing sleep on one occasion for many hours—when he awoke quite well. Dr. Alison's last attack of illness was on the 20th of August, when he had six severe fits, and some of a slighter character. On the 23rd, he had the usual doses of opium and antimony, producing sleep, after which he recovered well, so that I felt no hesitation in fulfilling my intention of going on my holiday, having arranged that my friend, Dr. Andrew Wood, should be sent for if requisite; and accordingly, on the 31st of August he was requested to visit Dr. Alison at Woodville, and ascertained that, although he had been occasionally out driving since I had visited him a week before, he had been a good deal excited, with occasional delirium. Dr. Andrew Wood, at his visit, found him quiet; but as he became excited towards evening, he got a dose of henbane, and twenty-five minims of solution of morphia, without, however, producing sleep, so that, next morning, the dose was repeated, and about 12 o'clock with success, and he awoke quiet, and during the evening continued well; but at 11 at night of the 1st September he became delirious, and this state continued during the whole of the following day, notwithstanding the administration of morphia, wine, &c., by Drs. Smith, Wood, and Brodie, who, from this time until the 22nd, when Dr. Alison died, were most assiduous in their attendance.

I have the advantage of minute notes, kept for me during my absence by one who never left our patient's side, and the record there, from day to day, indicates the frequent changes from comparative quietness to complete maniacal excitement, until within a few days of dissolution, when my medical friends remarked the similarity betwixt Dr. Alison's state and that of a person sinking under an attack of typhoid fever. There were brief intervals towards the close, when our patient was sensible, was able to express his thanks to those around him in his usual calm manner, and when he stated his belief that his end was approaching. But these peaceful moments were soon followed by excitement, characterized by violent spasmodic action and screaming, accompanied by a peculiar rotatory movement of the hands, or by a condition very frequent in the earlier attacks, at a certain stage of the seizure, when he seemed lost in the contemplation of some blessed vision, during which he expressed his belief that he heard the praises of the heavenly hosts, and that,

amongst the number, he distinctly recognised the voices of very dear departed friends.

At this period of his illness, as upon former occasions, it was truly interesting to note the beautiful expression of his intellectual countenance, as he listened with rapt attention to those sounds, of the reality of which he, at the time, entertained no doubt. His return to consciousness from this state, he more than once characterized as a return from heaven to earth. Towards the afternoon of the 22nd September his breathing became gentler and slower, and without the slightest struggle he passed away.

On my return home on the evening of that day from my holiday, I received, at the same time, the tidings of the illness and death of my much-esteemed friend and patient; the too considerate thoughtfulness of my friend and substitute, Dr. Andrew Wood, having led him to withhold the information of Dr. Alison's illness, from the fear that, were I made aware of his state, I would shorten the period of rest I had allotted to myself, and hasten to be with one of whom I had seen so much during the last ten years.

The post-mortem examination was performed about twenty-two hours after death, in presence of Professor Goodsir, Drs. Smith, Andrew Wood, Skae, Brodie, W. T. Gairdner, Haldane, and myself. Dr. Haldane has kindly furnished me with the following account of the appearances:—

*External Appearances.*—The body generally, and more particularly the face, were much emaciated. The cadaveric rigidity was generally moderate, but the muscles of the neck were so firmly contracted, that they supported the head when the pillows were drawn from below it.

*Head.*—The scalp was thin, and almost perfectly bloodless. The skull-cap was of moderate thickness; there were some pretty firm old adhesions between it and the dura mater. On proceeding to remove the dura mater, it was found that the falx cerebri was very extensively ossified; its anterior portion (almost quite up to its insertion into the crista galli), for an extent of rather more than three inches, being converted into a firm, bony mass, which had a rough irregular surface, presenting elevations and depressions which had corresponded to the sulci and convolutions of the adjoining portions of brain; posterior to this large mass there was a small ossification (about half an inch in length by a quarter in depth). On removing the dura mater, a quantity of clear serous fluid escaped from the cavity of the arachnoid. The membrane of the brain had a semi-opaque, milky appearance; there was considerable œdema of the pia mater, and much serous fluid upon the surface of the brain. The brain was removed before the ventricles were opened into, so that the quantity of serum contained in them could not be exactly ascertained; but as each lateral ventricle was much dilated, they must have contained at least two ounces of serum. The cerebral substance was healthy, though the walls of the lateral ventricles were rather soft. The cerebellum and pons were natural.

The weight of the encephalon was 52 oz. av. There was considerable ossification of the arteries at the base of the brain.

*Thorax.*—There were some old cellular adhesions on each side of the chest. Both lungs were a little emphysematous anteriorly; congested and œdematous posteriorly.

The heart weighed 14 oz.; its muscular substance was soft and flabby. The aortic valves were natural; the mitral valve was contracted, barely admitting the points of two fingers; its edges were thickened.

There was a moderate degree of atheroma of the ascending aorta.

*Abdomen.*—The substance of the liver was natural. The gall-bladder was completely filled with gall-stones of an irregularly cubical form, presenting smooth facettes; they consisted of cholesterine coloured with bile pigment. There was not a trace of bile in the gall-bladder, but a small quantity of a whitish, glairy mucus.

The cystic duct was obliterated. The other ducts were natural.

Each kidney was closely surrounded with fat, which also extended to a considerable extent into the pelves of the organs. The glands were rather smaller than natural; but, with the exception of pretty numerous cysts, which varied in size from that of a small pea to that of a pin's point, were sound in structure.

No other lesion was found.—*Edinburgh Medical Journal*, January, 1860.

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*Congenital Inequality of the two Sides of the Body; with remarkable Physiological Consequences.* By DR. PAUL BROCA, Professeur agrégé to the Faculty of Medicine; Surgeon to the Hospitals of Paris, &c.

I WAS recently consulted, during a short provincial tour, in the case of a child who presented a singular malformation.

The subject of this paper is a boy aged 11, in whom the two halves of the body are unequally developed. This inequality, observed by the parents since the first months of infancy, has become more and more apparent in proportion to the child's growth, but as no accurate measurement was made prior to my examination of him, I cannot say whether the inequality has actually augmented; I am, on the contrary, inclined to believe that the relative difference of the two sides of the body has always been the same, although the absolute difference has increased in proportion to the general development.

The left half of the body is remarkably more developed than the other. One would say that the body of the child is formed by the union of two halves belonging to two individuals of different ages, size, and strength. I shall first state the results afforded by the measurement of the length of the limbs. I may add, once for all, that the differences in volume correspond to those in length, and are even more decided[a].

[a] I have retained the French measurement, as showing the difference more precisely than would be the case if I had reduced the centimètres to English inches.—TRANSLATOR.

| | Left Side. | Right Side. | Difference. |
|---|---|---|---|
| *Lower Limbs:—* | *c.* | *c.* | *c.* |
| From the spine of the ilium to the internal malleolus, . | 65·5 | 60 | 5·5 |
| From the spine of the ilium to the upper edge of the patella (thigh), . . . . . | 32 | 29·5 | 2·5 |
| From the upper edge of the patella to the internal malleolus (leg), . . . . . | 33·5 | 30·5 | 3 |
| Length of the foot from the posterior extremity of the os calcis to the anterior extremity of the great toe, . . | 22 | 21 | 1 |
| *Upper Limbs:—* | | | |
| From the acromion to the styloid process of the radius, . | 41 | 39 | 2 |
| Length of the clavicle, . . . . . | 11·7 | 10·6 | 1·1 |

The shortening of the right lower limb by five and a half centimètres would in itself be sufficient to produce a considerable degree of lameness; but, in addition, the spine of the ilium of the left side is depressed by nearly two centimètres, and the lameness is thereby increased. This depression is not accompanied by any projection forwards or backwards, or with any of the deviations consequent on the lengthening or shortening of limbs affected with coxalgia of long standing. It seems to proceed exclusively from the unequal development of the two sides of the trunk. It will be observed, that if the obliquity of the bi-iliac line was consecutive to the shortening of the right limb, it would take place in an inverse direction, as the child, in walking and standing, is disposed to incline his body towards the shorter limb.

A perpendicular, let fall from the spine of the ilium on the median line of the abdomen, is shorter by a centimètre on the right side than on the left. The right great trochanter is less prominent; the right half of the scrotum is less voluminous than the left, although the two testicles appear to be of the same size. A similar inequality appears to exist between the two halves of the chest. Finally, the sterno-mastoid muscle of the right side is a little larger than the left, though it does not appear shorter; the neck and the vertebral column are perfectly formed, and present no deviation. There is no appearance of torticollis.

The most curious peculiarities are those which result from a comparison of the two sides of the face. The left eye is more open than the right. The external commissure of the eyelids is nearer the middle line of the nose on the right than on the left. The difference is nearly three millimètres. A line let fall vertically from this external commissure on the inferior edge of the lower jaw measures on the left side 8·5, against 7·6 only on the right. The difference is consequently 9 millimètres [rather more than the third of an inch].

The distance from the external angle of the eye to the lower margin of the ala nasi is five centimètres on the left, and only four and a half on the right side. The superior and inferior dental arches describe a longer curve on the left than on the right, and the teeth on the latter side are, for want of space, very irregularly inserted.

Lastly, and especially, the right half of the cranium is remarkably less developed than the left. A cord placed round the head on a level with the eyebrows, and passing on each side above the ear to meet the external occipital protuberance, measures 48 centimètres; of this length, 25 centimètres belong to the left side, and only 23 to the right, as appears from partial measurements, taken successively, of the two sides between the occipital protuberance and the nasal spine.

These measurements cannot be considered as rigorously exact, because it is almost impossible to fix with precision the median line of the occipital bone, but the difference of two centimètres is too considerable to admit of its being attributed to this source of error. We may, therefore, affirm that the two halves of the skull are unequally developed. Is this inequality entirely external, or does it coincide with an analogous arrangement of the two halves of the encephalon? This question I cannot decide, and still there are some physiological facts which would favour the latter supposition.

Although I had not time to investigate minutely, by means of pricking both sides of the body, the state of general sensibility in its several parts, this sensibility appeared to me to be less fully developed in the right than in the left limbs. The study of the functions of the organs of sense has yielded me much more satisfactory results.

The left ear being covered with the cap, and the right ear uncovered, the child was able to follow the ticking of my watch to the distance of $1\frac{1}{4}$ mètres [rather more than four feet]. Having passed this distance, he ceased to be able to mark with his hand the ticking of the seconds. With his left ear, on the contrary, he heard the ticking perfectly at $2\frac{1}{2}$ mètres, and even beyond that distance. Hearing was consequently much more perfect on the left than on the right side.

I made no comparative examination of the sense of smell possessed by the two nasal cavities. This investigation, very difficult under any circumstances, would probably, in so young a child, have led to no decisive result, and it would have required more time than I had at my disposal. It would have been still more difficult to study comparatively the special sensibility of the two sides of the tongue. This I propose to do in a few years, when the child shall be older and more capable of describing his sensations; but I may even now state that the left half of the tongue is very decidedly larger and particularly thicker than the right. On making the patient protrude the tongue as far as possible, and measuring the breadth of the two sides of this organ on a level with the dental arch, the breadth of the right was found to be 1·66 centimètres, and that of the left 2·33, giving a difference in favour of the left of 0·67 centimètre. The median line of the tongue is consequently inclined to the right, and the inequality of the two halves is so well marked that an abrupt antero-posterior relief indicates the limits of the left side. It is a state of things quite similar to that which in time supervenes as a result of hemiplegia.

With respect to vision, the child had already perceived that his

right eye was much better than the left. To make sure that this result did not depend on myopia of the left eye, I caused him to close the right eye, and I ascertained that the left is as weak with respect to near as it is with distant objects. So far as I can rely upon the statements of a child of his age, I think I established that the images of the left eye appeared at once less distinct and less illuminated than those of the right. However this may be, it is beyond all doubt that the vision of the left side is notably inferior to that of the right.

This fact is the more curious, as the better eye corresponds exactly to the worse ear, and to the less developed of the two halves of the cranium, the face, the trunk, and limbs. How is this to be explained? It seems to me difficult not to refer it to the decussation of the optic nerves. This explanation is doubtless far from satisfactory, since the decussation is only partial, and each eye receives nervous fibres proceeding from both optic bands; nevertheless, I am disposed to believe that the cause of the inequality of the two eyes resides neither in the eyeballs nor in the optic nerves, but more posteriorly, whether in the optic bands, or in their central origin.

When the patient's intellect shall be fully developed, it will be interesting to explore separately the several parts of each retina, and perhaps we shall be able to establish phenomena adapted either to complete the history of this particular case, or to throw some light upon the question, still so obscure, of the respective distribution of the direct or decussated nervous fibres in the several regions of the retina.

One word, finally, as to the child's intelligence. He belongs to a family very remarkably gifted in this respect; although at least as advanced as most of his companions, he is less so than his numerous brothers and sisters were at his age; but this is in my mind unimportant.

The cause of the malformation just described is entirely unknown. I can assert that it is not hereditary, for I have long known all the child's family.

The parents consulted me chiefly as to the treatment of the lameness, which increases with the growth of the child. I have advised a system of gymnastics calculated to bring specially into play the limbs of the right side. I have particularly recommended that the child should play at turning, taking care that he should always move the pedal with the right foot. I do not venture to hope that this will have the effect of lengthening the limb, but I hope it may at least tend to develop the muscles, which are considerably weaker and more more slender than those of the opposite side[a].—*Journal de la Physiologie de l'Homme et des Animaux, publié sous la direction du Dr. Brown-Séquard*, No. 5, January, 1859, p. 70.

[a] Experiments which I have made on cats and guinea-pigs show how much reason my learned friend, M. Broca, has to hope that galvanism will cause the development of the muscles of the right limbs of the child. He will permit me to add, however, that if he had been aware of the results of these experiments, he would,

*On Indigo as a Test for Grape and Fruit Sugar.* By DR. E. MULDER.

GRAPE and fruit-sugar possess the property, when alkalies are present, of reducing indigo-blue to indigo-white. If one of these two kinds of sugar be dissolved in water, and indigo, and afterwards potash or soda, be added to the solution, the indigo is, with the aid of heat more rapidly than at the ordinary temperature, reduced to indigo-white, which is soluble in alkalies. The same takes place if alcohol be used instead of water; in this case the reduced indigo is deposited, not in the amorphous, but in the crystalline state (Fritsche), the reduction still taking place, though more rapidly.

If a solution of indigo (indigo in combination with sulphuric acid), and not indigo in powder, he employed, we have a means of detecting the most minute traces of grape and-fruit sugar. When indigo is employed in solution, the reduction takes place more readily, the indigo, which is in itself a tolerably stable body, being by combination with sulphuric acid converted into a more easily changeable body.

In the reduction of sulphate of indigo by grape and fruit-sugar, there is one condition which it is indispensable to fulfil, that is, the solution must be alkaline. But the indigo solution is not only easily reduced, the indigo is also oxidized; by caustic potash or soda it is wholly or partially decomposed. In the former case the solution acquires a yellow colour, which, after saturation with an acid, is again converted into blue if the solution was not previously heated; in the second case the colour of the solution is green, and is little altered by boiling, provided the ebullition is not too long continued. If an excess of carbonate of potash or soda be added to the indigo solution, the blue colour continues, even after boiling, nearly unaltered.

Both the green and the blue solution afford an extremely delicate test for grape and fruit-sugar. The first is more sensitive than the second, and yet we give the preference to the latter.

In seeking for grape and fruit-sugar it is desirable to avoid any reagent, which with great facility decomposes other organic bodies. Now it is a property of the green solution that it decomposes other organic bodies, and therefore it does not deserve to be recommended as a test for grape and fruit-sugar.

That this does not apply to the above-mentioned blue solution is shown, for example, by the fact that it is not capable of decomposing cane-sugar, even when it is boiled with it for some hours; but so soon as a trace of grape-sugar is added to the solution, the decomposition immediately takes place. The green solution, on the contrary, is decomposed by cane-sugar.

The decomposition by grape or fruit-sugar of a solution of indigo

perhaps, have ventured to hope also that the influence of galvanism may even go so far as to induce lengthening of the limb. I have, in fact, seen in young animals limbs completely paralyzed become, under the action of galvanism, as much developed, with respect to their structure, their dimensions, and the vital properties of their muscles, as healthy limbs.—*Note of M. Brown-Séquard.*

rendered alkaline by carbonated (or caustic) alkalies, presents so much that is peculiar, that we must dwell upon it for a while. In the first place, we must inquire what takes place in solution of indigo in sulphuric acid[a], after the addition of carbonated alkalies, for example, of carbonate of potash. We have formed indigo-sulphate and indigo-hyposulphate of potash, while the excess of sulphuric acid enters into combination with potash; at the same time the solution is rendered alkaline by the carbonated alkali. Indigo-sulphate of potash is, at the ordinary temperature, soluble neither in sulphuric acid nor in carbonate of potash; but on the application of heat it is dissolved, or at least becomes so finely divided in the fluid, that it may be looked upon as dissolved. But indigo-hyposulphate of potash remains in solution at the ordinary temperature, so that by filtration it can be separated from the indigo-sulphate.

But if we really desire an alkaline solution of indigo, we must take the latter. We shall then do well to dissolve the indigo in fuming, but not in English sulphuric acid, giving rise to acid hyposulphate of indigo. This is, however, not necessary, as indigo-sulphate of potash is as delicate a test of the presence of grape and fruit-sugar as the indigo-hyposulphate.

We have, therefore, in solution, indigo-hyposulphate of potash, and, minutely subdivided in the solution, indigo-sulphate of potash, which, on the application of heat, is partially dissolved. A portion is placed in a test-glass, and so much water is added as may be necessary to afford a light blue solution, which is boiled. The colour of the solution now remains nearly unaltered, as has already been mentioned, which is a very desirable property. If now, after the boiling, an extremely small portion of grape or fruit-sugar be added, the colour becomes first green, and then purple red; if the quantity of sugar was greater, the purple red passes gradually into red, and finally into yellow. If the purple red fluid be shaken, it becomes green, and then blue; but if it was yellow, it passes through the above-mentioned colours, from yellow to greenish-blue or green.

The same experiment may be repeated as long as there is sugar in the solution, and as we shall soon see, as long as there is still indigo in the solution.

I should have been glad if I could have expressed all these shades of colour in chemical formulæ, and have added a satisfactory explanation of the numerous changes which take place in this reaction. I believe, however, that the following attempt at explanation will not be far from the truth.

Starting from the supposition that a great analogy exists between the different coloured products obtained by Berzelius by the action of lime-water on indigo-sulphate of potash, and those which I observed in the indigo-sugar reaction, I have arranged my investiga-

[a] In the preparation of this solution ordinary indigo may be used. The solution, after having been diluted with water, is filtered, whereby, except the indigo-purple, all impurities are separated.

tion accordingly, and have obtained a result really in accordance with my hypothesis. Indigo-sulphate of potash may be boiled without perceptible change of colour with carbonate of potash; but if the boiling be long continued, we obtain a yellow colour, as in the sugar reaction. This yellow colour is due to a product of decomposition in the indigo, which quite agrees with that obtained by Berzelius when he acted on indigo-sulphate of potash with lime-water (flavin-sulphuric acid). I shall return more particularly to this body; for the reaction itself it is sufficient that it arises without sugar from the effect of carbonate of potash on indigo-sulphate of potash. In the sugar test, therefore, the solution must finally become yellow[a], and all the indigo must eventually be decomposed.

But from the indigo-sulphate of potash another product of decomposition with a red colour (rufin-sulphuric acid of Berzelius) may also be formed. The red colour occurs before the yellow, so that the latter proceeds from the former.

If we bring these facts to bear on the reaction, we may deduce the following. On boiling the blue solution, a red, and probably from this partly already a yellow body, is immediately formed by the action of the carbonate of potash on the indigo blue; on account, however, of the very small quantity, the colour of the fluid is but little changed. If we now add grape-sugar to the solution, the indigo-blue is reduced to indigo-white, upon which the red colour becomes apparent. The latter has, therefore, nothing to do with the reaction of the sugar. If we now shake the fluid, the indigo-white is converted into indigo-blue, and the colour appears to return but little altered; nevertheless, it is mixed with some red, and probably also with some yellow. On repeating the experiment, we finally obtain a yellow solution; the red body is converted into the yellow. If the fluid is now agitated, it begins to assume a red, bluish-green colour, as in the same moment when a portion of the indigo-white passes into indigo-blue, another portion of solution is converted by the carbonated alkali into the red body. The solution must now have a green colour, for what is once yellow continues yellow, and blue with yellow forms green; the red body passes again into the yellow. From the foregoing the red and green colour on the surface are easily explained.

The experiment might therefore be infinitely continued while sugar is present, were the indigo not decomposed by carbonate of potash. On the sugar test this decomposition has no influence; it communicates a colour to the reaction, which may be interesting to the chemist. But I again repeat that the beautiful series of colour. has nothing to do with the reduction of indigo.—*Archiv für die Holländischen Beiträge zur Natur- und Heilkunde*, Band 2, Heft 1. p. 44. 1858.

[a] It should here be mentioned, that yellow products of decomposition are obtained also by the action of carbonate of potash on sugar; but these do not occur with the indigo test, unless the sugar be present in excess.

*On the Forms of Disease affecting the Labourers in the Quicksilver Works of Idria.* By JOS. HERMANN, Senior Physician to the Syphilitic Department of the Imperial and Royal Hospital at Vienna (*Med. Wochenschrift* 1858, Nos. 40, 41, 42).

ACCORDING to the statements of Dr. Görbez, Physician at Idria, out of 516 workmen, occupied partly in mining, partly in smelting, and partly in the cinnabar manufactory, 122 were in 1856 affected with dyspepsia, ptyalism, scrofula, anemia, neuralgia, mercurial gout, mercurial tremor, and caries. The labour is divided among the miners in such a manner that 59 men, who are changed monthly, are assigned to the smelting house, and 9 to the cinnabar manufactory. The occupation in the shaft consists in procuring the ore; in the smelting-house the ore is heated in the furnace, and the quicksilver is reduced to the metallic state; the cinnabar is obtained by subliming the æthiops mineral. Idria itself possesses favourable climatic and social conditions, and its high mortality (120 annually out of 4500), and the evident sickliness of its entire population, are to be ascribed to the influence of the quicksilver. The immediate action of the mercury takes place, according to the author, in the first instance from the fine dust containing quicksilver (from the sweeping of the tubes, &c.), which becomes deposited on the mucous membrane of the mouth and the outer integuments, and produces intoxication. To what an extent this may occur is most plainly proved by the fact that not unfreqnently globules of quicksilver are found in the bathing-tubes which have been used. Intercommunication between the affected workmen and the rest of the population, and the air of the valley impregnated with the mercurial vapours, are additional sources of disease. In the second place, particles of dust and vapour are imbibed through the organs of respiration and digestion. This injurious influence extends also to *animals;* thus cows, which feed in the neighbourhood of the furnaces, suffer from ptyalism, become cachetic, and abort, the calves themselves are sick; trout, too, which are kept in reservoirs in Idria lose their red spots, when hot scoria are thrown into the river. As prophylactics against hydrargyrosis, the author recommends an instrument, similar to a respirator, for closing the mouth, new milk, cod-liver oil, chewing tobacco or sage, frequent baths, and change of the workmen's clothes; as remedies, iodide of potassium and diaphoretics, fresh, pure air. As to iodide of potassium, according to the experience of Dr. Guillot and Professor Melsen, it enters into double combinations with all compounds of mercury which can possibly be met with in the animal body, and affords as good a remedy, as it does a valuable preservative, against poisoning with quicksilver. Iodide of mercury can be detected in the urine of those affected with hydrargyrosis who have been treated with iodide of potassium. The author infers from the further statements of Dr. Görbez, and partly from his own observations, that in quicksilver labourers in Idria there occur, occasioned by the occupation of the patients, maculated, papular, vesicular, pustular exanthemata;

chronic, kidney-shaped ulcers; angina, erosions, and ulcers of the mucous membranes; swelling, inflammation, and wasting of the glands; intumescence of the liver, with all its consecutive phenomena—atrophy of the liver, gastralgia, prosopalgia, ischialgia, nocturnal pains in the bones, periostitis, osteitis, necrosis, caries, a state similar to chlorosis; lastly, scrofula in all children of parents affected with hydrargyrosis. A case is specially quoted, in which the children begotten in Idria were scrofulous, while those previously born of the same parents in other places were not scrofulous. Mercurial tremors stand in an inferior numerical proportion. The important question, whether some diseases occurring among the miners in Idria, hitherto ascribed to syphilis alone, belong to syphilis or to hydrargyrosis, or to a mixture of both—the author answers, that they are attributable solely to the influence of quicksilver. Primary syphilis is an extremely rare disease in Idria, and as the smelters constitute a fixed population, it can be positively ascertained that hereditary syphilis is not connected with these affections.

Professor Mitscherlich, of Berlin, has stated, in a communication to Dr. A. Reumont, of Aix-la-Chapelle (Frorieps Notizen, 1858, No. 21), that, according to his observations in reference to the miners in the quicksilver works in Idria, he has not seen among them a single case of periostitis and swelling of the bones, and Dr. Zerbei, physician there, has confirmed the same from his lengthened practice, adding, that he has observed neither hyperostosis, cervical abscesses, nor exanthemata. According to the last-named writer, the so-called arthritis mercurialis is in more advanced life developed in those workmen who expose themselves to the vapours of quicksilver, to changes of temperature, and to damp. According to Falck (Virchow, Handbuch, ii. 1), there is, however, no doubt that the osseous system may be affected by mercury, either in consequence of ulcerative or gangrenous destruction, which extends from the gums to the alveoli, or commences in the periosteum of the cylindrical bones, or in the light bony tissue of the base of the skull. Ricord is in favour of the purely syphilitic origin of tertiary forms, appealing to the rarity of affections of the bones among quicksilver labourers, an observation previously made and maintained by Ramazzini.—*Vierteljahrschrift für die praktische Heilkunde*, 1859, Band 62, part 2, page 27.

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*A Contribution to Morbid Urology.* By WILHELM BRATTLER.

DR. BRATTLER'S essay comprises the results of numerous investigations, relating chiefly to the following diseases: typhus, measles, scarlatina, the effects of inoculation with glanders, acute rheumatism, pneumonia, pleuritis, Bright's disease, diseases of the heart, Asiatic cholera, pulmonary tuberculosis, syphilis, cancer of the stomach, and emphysema of the lungs, with cerebral apoplexy. The author confined himself to the estimation of the amount of urea, of chlorides, and of phosphoric acid in the urine, and made use of Liebig's volumetric methods of analysis. Moreover, he examined only acid urine

and almost exclusively the urine of men, as the urine of women is for several reasons, not well adapted for examination. He prefaces his investigations with the average numbers obtained by other observers, as representing the amounts excreted in healthy individuals, whence the daily quantity of urine appears to be 1563 cubic centimetres; that of the urea to be 31·583 grammes; of the chlorides, 13·32 grammes; and of the phosphoric acid, 3·603 grammes. At the same time, he refers to the experiments of other writers, showing that in the physiological state the solid constituents of the urine are increased by the abundant use of highly nitrogenous food, by copious drinking, active bodily exercise, and exertion. He subsequently passes to his investigations made in the above-named diseases:—1. *Typhus.*—As in this disease the excretion of urea is excessively increased, although but little nourishment is taken, the increase in question can proceed only from an exalted metamorphosis of the nitrogenous constituents of the body accompanying the fever, whence the high degree of emaciation of typhus patients is explicable. It further appears that the more considerable the increase of temperature is (in the first two weeks), the greater is the excretion of urea, and that, with a sinking of the former, a decrease of the latter is observed, The diminution of the excretion of urea during convalescence is explained by the conversion of the nitrogenous food into organic histological elements. The chlorides appear to be diminished in the urine of typhus, and this diminution is a result—*a*, of the small supply of food, with probably diminished absorbent power of the intestinal canal; *b*, of diarrhœa; and *c*, of pulmonary exudations. The *phosphoric acid* in the urine of typhus is circumstanced exactly like the urea, and as the muscles are very rich in phosphoric acid, and waste greatly in violent febrile movements, the amount of the acid depends upon the metamorphosis of these organs. The prognosis is bad in those cases in which albumen occurs in the urine. The author found the urine in typhus always acid, and he rarely observed a deposit of urates. The urea and chlorides afford a prognostic indication in typhus, for those cases where the excretion of urea has not attained any very great height, and where the chlorides do not long continue diminished, run a favourable course. From the observations quoted (including nine cases), it would follow, as a therapeutic indication, that our object should be to replace the nitrogenous constituents of the body, and to moderate the fever. The administration of good, highly salted broth, with albumen and *café noir*, quinia (from twelve to eighteen grains in twenty-four hours), and when the temperature of the body reaches to 102·2 F., cold baths, a treatment which is said to have been for years approved in the clinique of Privy Councillor v. Gietl, will fulfil these indications. 2, *Measles.*—Only one case was observed, in which during the eruptive stage but very little urine was voided, and the solid constituents were diminished. In the stage of full efflorescence the secretion of urine was abundant; the urea was very much increased; the phosphoric acid was but little diminished; the chlorides were for three days wholly absent. In convalescence there was diminution of the

urea and of the phosphoric acid, with increase of the chlorides. 3. *Scarlatina* (one case).—In the stage of efflorescence the secretion of urine was diminished: in the stage of declination it was increased. The urea was considerable during the efflorescence; the phosphoric acid was slightly increased; the chlorides were, in proportion to the nourishment taken, abundantly excreted. In convalescence the urea and phosphoric acid were rather below the normal amount. 4. *Inoculation with Glanders.*—The quantity of urine was normal; the urea and phosphoric acid were increased; the chlorides were diminished. 5. *Acute Rheumatism* (four cases).—The quantity of urine was diminished at the commencement and during the height of the disease, and increased with the subsidence of the fever and of the affection of the joints. The tendency of the urine to throw down urates disappeared when the amelioration set in. The specific gravity, which was very high while the urine was scanty, fell with the increase of the secretion. The urea was increased at the commencement and during the height of the disease, and diminished with the decline of the fever. The chlorides exhibited an opposite behaviour. Their diminution coincided with the formation of exudations. The phosphoric acid presented no definite relation. 6. *Pneumonia* (two cases). —The urea appears to be considerably increased in the stage of hepatization, and gradually sinks with the occurrence of resolution and the decrease of the fever to beneath the normal proportion. The chlorides, as is well known, exhibit the opposite condition. The phosphoric acid behaves like the urea. The increase of it and of the urea is attributable to the strong febrile action, while the decrease of the chlorides is due to the large amount of common salt present in the pneumonic exudation. 7. *Pleuritis* (two cases).—The quantity of urine, which is at first diminished, considerably increases with the resorption, and the latter terminates with the formation of deposits. The urea and chlorides are similarly circumstanced, but the phosphoric acid is diminished during the whole course of the disease. 8. *Bright's disease* (two cases).—In the commencement the urine contains a large amount of albumen and of blood, and is itself diminished in quantity; the urea and phosphoric acid are also diminished; and the chlorides are reduced to a minimum. If the patient improves, and the dropsy decreases, the quantity of the urine and of its solid constituents becomes enormously augmented. When the dropsical fluid is completely absorbed, the urea, chlorides, and phosphoric acid sink to about the normal amount. If no improvement takes place, the amount of urea steadily decreases, while the quantity of urine may be normal, or even in excess. The increase of the urea during the resorption of the dropsical fluid is to be attributed to the elimination of the portion of that principle retained in the blood and separated with the transudations; it may be, too, that the nitrogen of the albumen of the serous effusions is changed into urea. The increase of the urine and of the chlorides is due to the resorption of the fluid. The author also examined the urine of two patients labouring under *valvular disease of the heart;* of one affected with *pericarditis;* of one *cholera* patient, of one *tuberculous* and one *syphi-*

*litic* individual; and of one suffering from *carcinoma, scurvy, and emphysema.* From all his investigations, with the addition of some urinary analyses by other observers, he infers that the urine in disease does not undergo any specific changes peculiar to the several morbid processes, but that its changes are connected with very definite occurrences in the system, and that, accordingly, the urine may exhibit a similar behaviour in different diseases. On the whole, it is established that the *quantity of urine is diminished*—1, in the commencement of almost all febrile diseases; 2, in diseases of the kidneys, in which the tubuli uriniferi are obstructed; 3, in those diseases where the system suffers great loss of water (cholera); 4, in diseases of the organs of circulation and respiration, in which the amount of blood supplied to the aortic system, and therefore to the kidneys, is lessened (lesions of the heart, pleuritis). The quantity of urine is increased in the resorption of dropsical effusions and exudations, in polydipsia, in diabetes insipidus. The urea is diminished in convalescence from all acute diseases accompanied by considerable loss of substance, in derangements of the digestive organs (typhus, carcinoma of the stomach), in diseases of the kidneys, which interfere with their functions, and in affections of the organs of circulation and respiration. The urea is increased in all diseases attended with fever in the resorption of dropsical effusions, and after the removal of the obstruction in those diseases where the urea, in consequence of a functional disturbance of the kidneys, is long detained in the blood. The *chlorides* are diminished in all diseases combined with exudations and transudations, in all diseases of the digestive organs which impede digestion, and in functional disturbances of the kidneys attended with diminished secretion of urine. Increase of the chlorides, on the contrary, occurs in the resorption of dropsical effusions. The *phosphoric acid* is lessened in renal diseases, in which the urinary excretion is decreased, and in affections of the digestive organs attended with disturbed digestion. On the other hand, it is augmented in acute febrile diseases, and in those affections where, in consequence of functional derangements of the kidneys, the phosphoric acid is long retained in the blood, and the obstruction is finally removed.—*Vierteljahrschrift für die praktische Heilkunde*, Band 62, 1859, p. 55.

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*On Polypi of the Œsophagus.* By PROFESSOR A. TH. MIDDELDORPF

UNDER the name "polypi" are, as is well known, comprised more or less stalked tumours of no definite structure, occurring on mucous membranes. Polypi of the œsophagus or pharynx, which, in comparison with those on other parts covered with mucous membrane, occur remarkably rarely, are in the stricter sense only such as take their origin in the œsophagus, but in a more extended sense include all those whose root, stalk, or body lies within the canal in question. The cause of the formation of polypi in the œsophagus is still obscure; Frerichs' theory is the most probable, that they are

the result of obstruction and hypertrophy of the mucous glands. On the other hand, the fact that œsophageal polypi are usually situated in that narrow space near the larynx, on a level with from the fourth to the sixth cervical vertebra, where the food, in order to reach the œsophagus, must pass, and where the contraction of the circular muscular fibres promotes the folding of lax mucous membrane, is in favour of the view which attributes them to increased relaxation, plaiting, and swelling of the entire mucous membrane. The structure from which the polypous mass springs has not as yet been accurately examined. The œsophageal polypus is drawn downwards and elongated, at first by the movements of deglutition, then by the food gliding over it, and, when it has attained a certain size, by its own gravity. It behaves in one respect in the same manner as rectal polypi, in another in an opposite mode; it constantly stimulates to movements of deglutition, while the sufferer from rectal polypi endeavours to expel them by straining. The pressure of the sphincter prevents the free development of the rectal polypus externally; the œsophageal polypus, on the contrary, grows quickly downwards. The œsophageal polypus extends the structure from which it originates in such a manner as to form a fold, which constitutes the stalk, without involving the parenchyma of the œsophagus in the polypus, as is often the case with rectal and uterine polypi. The ordinary *changes* which the polypus undergoes consist in exudation, suppuration, and hemorrhage, as results of excessive swelling, or contraction of the œsophagus, or of exfoliation of the epithelium. The parts of the œsophagus surrounding the polypi are inflamed with catarrhal or purulent discharge. Among the *symptoms*, dysphagia occupies the first place; it is usually greater when the air is dry, and with the growth of the polypi augments and becomes more distressing, being finally combined with pain, dyspnœa, sensations of choking and suffocation. In addition, nutrition is much interfered with, in consequence of the impediments to taking food, and of occasional bleedings from the tumour or from ulcers of the œsophagus. Almost invariably œsophageal catarrh co-exists, and sometimes bloody and purulent sputa are expectorated. Among the phenomena most important in reference to diagnosis are nausea, efforts at vomiting, with rising of the polypus into the throat, the cavity of the mouth, or even between the teeth. The diagnosis is not certain until the polypus is visible and within reach of the fingers. Examination with the œsophageal tube or with the probang may be deceptive; still, sounding is to be recommended on account of the efforts at vomiting it produces, which may drive the polypus into the cavity of the mouth. If the polypus is deeply situated, or, if it is not large enough to be brought by vomiting into the cavity of the mouth, we may yet hope to catch it by sounding with a wire noose. If the polypus is neither seen nor felt, the diagnosis is very difficult, and is possible only approximatively by exclusion. The *prognosis* is unfavourable, for the polypus is seldom spontaneously thrown off, and, if it is not removed, or cannot be removed, it proves fatal by starvation and im-

peded respiration. The *treatment* is attended with great difficulties. Nutrition and respiration are to be maintained by palliative measures. Strong fluid nourishment and oleaginous and mucilaginous drinks are to be given, if necessary, by means of the stomach-tube. Gastrotomy or enterotomy would scarcely be justifiable, although they have been proposed when no more food could be introduced into the stomach. Should the respiration be very much obstructed by the polypus, bronchotomy might be had recourse to. Operation for the removal of the morbid growth can be successfully employed only when the polypus reaches into the cavity of the mouth. Under other circumstances it is hard to seize it. The operation may be performed in different modes. Twisting or tearing off the polypus is in general inadmissible; cutting it off with a long scissors with curved blades is very dangerous, on account of the subsequent hemorrhage. The ligature may be applied in three modes:—1. According to Bell, by means of a wire loop introduced through the nose; compression is then produced by means of a tube passing over the ends of the loop. 2. Directly on polypi which have got into the cavity of the mouth. 3. According to Dallas, the polypus may be again drawn down as in swallowing, after the ligature has been applied. In the first case the application of the ligature is very difficult, and scarcely answers the purpose; in the second, great difficulty of breathing may set in, and bronchotomy become necessary; in the third case bronchotomy may also be necessary to prevent suffocation. It is therefore most advisable to combine direct removal with the ligature. The latter prevents the hemorrhage; the former wards off the suffocative symptoms, and obviates the necessity for bronchotomy. For removing the polypus the author recommends the galvanic cautery after the application of the ligature. The hemorrhage after excision without ligature may be very considerable and difficult to stop. Ice and styptics are scarcely sufficient to stop the blood; compression with sponge or a piece of indian-rubber is better. Œsophagotomy is too dangerous to be recommended. In conclusion, the author details a case of operation performed in January, 1853, which is at the same time the first where the galvano-cautery was employed, and in which the removal of the œsophageal polypus succeeded.—*Vierteljahrschrift für die praktische Heilkunde*, Band 62, part 2, 1859, page 50.

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### *On the Influence of Dilution of the Blood upon the Secretion of the Urine*[a].

By DR. MAX HERRMANN, of Berlin.

As it has been shown that on increase of the pressure of the blood albumen is excreted with the urine, it must be interesting to exa-

[a] The experiments on which this Essay is based, were performed in the Pathological Institute of Berlin, with the kind assistance of Dr. Hoppe, and have been detailed in my inaugural dissertation. The object of the present communication is to give a shorter exposition of the same.

mine in what relation the concentration of the blood-plasma stands to the elimination of albumen by the kidneys, that is, whether dilution of the blood, without essential change, or, at least, without increase of tension in the glomeruli, is followed by a transudation of albumen into the tubuli uriniferi. Experiments of this kind have been made by Kierulf[a], who injected considerable quantities of water into the bloodvessels, and partly by previous abundant abstractions of blood, thought he satisfied himself that the results obtained did not depend on increased tension of the blood. The results, according to his statement, consist in this, that after the injection has taken place, "first, albumen, and then a true bloody urine is excreted." As, however, the proof of the proposition, that, under these circumstances, albumen is excreted without hematosin, can be derived only from an experiment, and the almost constant presence of blood-cells in the excretion designated as bloody urine, admits of the supposition of a complication of causes, it appeared that the effect of the dilution of the blood as such required to be still more accurately investigated. Last summer I instituted numerous experiments upon this subject, but in no case could I, after the injection of considerable quantities of water, find albumen or hematosin in the urine; on the other hand, the colouring matter of the bile was almost invariably present. The experiments were performed on dogs, and the urine was obtained partly by shutting up the dogs in cages, and partly by the establishment of urinary fistulæ. The operation was performed beneath the kidney, between its inferior margin and the superior spine of the ilium, so that, in looking for the ureter I was obliged to start from the very hilus of the kidney. The constant result of these manipulations was an exceedingly slow and sparing excretion; sometimes, even, the urine ceased for hours to be eliminated. It was not at that time my object to make accurate determinations by weight; the measurement of the urine spontaneously voided within twenty-four hours showed, however, a considerable increase.

On repeating these experiments, the urine was always collected by means of a urinary fistula, and the latter was established as near as possible to the opening of the ureter into the bladder. For the description of the operation and of the apparatus for collecting the urine, I must refer to the communications already made, especially as the manipulation does not differ essentially from Kierulf's. By taking all precautions we might be sure that the flow of urine from the ureter continued quite normal, and that the estimation of the quantities of urine excreted was, at all events, liable to no errors of any consequence.

With a view of ascertaining the result of the dilution of the blood in the directions above indicated, the following experiments were performed:—

[a] Henle und Pfeuffer's Zeitschrift. N. F. Bd. 3. Page 279.

*Experiment I.*—A ureter-fistula was established in one side of a medium-sized dog; the operation did not last long, and was unattended with any loss of blood worth noticing. After the urine had been collected for a time, to observe its nature and the rapidity of its flow, the external jugular vein was laid bare, from $12^h\ 41'$ until $1^h\ 15'$, and 350 cubic centimètres of distilled water, at 95° Fahrenheit, were injected.

| Time of Collection. | Quantity of Urine per Minute. | Observations. |
|---|---|---|
| H. M. — H. M. | | |
| 10 25 — 11 35 | 0.116 | The urine was clear and yellow, and contained neither albumen nor colouring matter of the bile. |
| 11 35 — 12 41 | 0·193 | |
| | Injection of the Water. | |
| 1 15 — 1 35 | 0·858 | The colour of the urine was not essentially altered; there was no albumen; but, on the other hand, the colouring matter of bile was present. |
| 1 35 — 2 0 | 0·829 | |
| 2 0 — 2 40 | 0·411 | |
| 2 40 — 3 8 | 1·053 | |
| 3 9 — 3 19 | 2·368 | |
| 3 19 — 3 43 | 1·081 | |
| 3 43 — 4 8 | 0·647 | |

*Experiment II.*—The fistula was in this case also successfully established. After the collection of some portions of urine, 600 cubic centimètres of distilled water were carefully injected into the external jugular vein.

| Time of Collection. | Quantity of Urine per Minute. | Observations. |
|---|---|---|
| H. M. — H. M. | | |
| 9 40 — 10 30 | 0·279 | Colour and composition normal. |
| 10 30 — 11 35 | 0·226 | |
| | Injection of the Water. | |
| 12 7 — 1 22 | 0·225 | The colour was perfectly golden yellow. There was a very beautiful reaction of biliary pigment; no albumen. |
| 1 23 — 2 8 | 0·303 | |
| 2 9 — 2 59 | 0.416 | |
| 2 59 — 3 31 | 0·555 | |

Immediately after the injection some blood was perceived in the glass tube connected with the canula; it was in the form of a slight streak, distinct from the otherwise clear urine. This impurity was, however, transitory; the urine again flowed light and clear, and exhibited no trace of albumen. After the death of the animal, clear yellow urine was also found in the bladder. The hemorrhage, therefore, can have proceeded only from the ureter. It is evident that without great care mistakes might easily be made.

*Experiment III.*—The operation was performed on a rather small dog.

| Time of Collection. | Quantity of Urine per Minute. | Observations. |
|---|---|---|
| H. M. H. M. | | |
| 8 57 — 9 53 | 0·090 | The urine was normal. |
| 9 53 — 11 30 | 0·091 | |

Injection of 550 cubic centimètres of Distilled Water at 95° F. into the external jugular vein.

| | | |
|---|---|---|
| 11 54 — 12 47 | 0·290 | The colour of the urine was light or dark red, depending both on the time, as also, as it appeared, on the rapidity of the discharge. The urine contained albumen and hematosin, but no blood-cells. |
| 12 47 — 1 15 | 0·700 | |
| 1 15 — 2 5 | 0·424 | |
| 2 5 — 3 3 | 0·466 | |
| 3 3 — 3 34 | 0·993 | |
| 3 34 — 4 4 | 0·943 | |
| 4 4 — 4 36 | 1·061 | |
| 4 36 — 4 58 | 1·001 | |
| 4 58 — 5 20 | 0·557 | |
| On the following day. | | |
| 12 40 — 1 9 | 0·619 | The urine was again yellow, and was not albuminous. |
| 1 10 — 1 20 | 0·798 | |

On the injection of the last cubic centimètre of water, the colour of the urine became red; the redness gradually diminished towards the end of the collection. Next morning the urine again flowed with its usual colour, without containing a trace of albumen; about noon a small quantity was collected to ascertain whether the acceleration of the excretion still continued.

*Experiment IV.*—The fistula was in this instance also established in a rather small dog.

| Time of Collection. | Quantity of Urine per Minute. | Observations. |
|---|---|---|
| H. M. H. M. | | |
| 8 41 — 9 10 | 0·505 | Normal urine. |
| 9 10 — 9 46 | 0·169 | |
| Injection of 300 cubic centimètres of Distilled Water at 95° F. | | |
| 10 4 — 10 37 | 0·144 | Neither biliary pigment nor albumen. |
| 10 37 — 11 15 | 0·106 | |
| Injection of 350 cubic centimètres of Distilled Water at 95° F. | | |
| 11 32 — 12 17 | 0·300 | Colour red, hematosin and albumen, no blood-cells. |
| 12 17 — 1 15 | 0·134 | |
| 1 15 — 2 11 | 0·066 | |
| 2 11 — 3 10 | 0·123 | |
| 3 11 — 3 35 | 0·935 | |
| 3 35 — 4 15 | 0·685 | |
| 4 15 — 4 42 | 0·709 | |

The injection was twice performed, in order to study more exactly the causes of the occurrence of apparently bloody urine. Neither in this case did the first portion of water produce the excretion of albumen. On this occasion, moreover, exceptionally, no biliary pigment was demonstrable. After the second injection the urine immediately became red, but was transparent as in the preceding experiment. No blood cells could be discovered.

*Experiment V.*—In this instance I cannot give numbers, as an unfortunate accident prevented the completion of the experiment. In order to obtain a still greater dilution of the blood, with an actually diminished pressure, a quantity of blood was abstracted previously to the injection. The urine, which had before been quite normal, was entirely suppressed after the abstraction of the blood, and it was not until long after the injection that it again flowed slowly out. It had become red, and contained hematosin and albumen, but no blood cells.

*Experiment VI.*—This was intended as a repetition in essential points of the fifth experiment. After the collection of some portions of urine, 350 cubic centimètres of blood were abstracted, and in place thereof 475 cubic centimètres of water were injected. The urine exhibited no apparent change of colour. A second injection was subsequently made; the quantity of water was larger than was intended, but it was necessary to obtain the discharge of red urine. Up to the end of the injection pauses were frequently made to watch the effect, the urine, however, had not changed, and it was not until the injection of the last cubic centimètre of water that the colour became reddish. The dog in which the fistula was established was, moreover, much larger than any of the preceding dogs.

| Time of Collection. | Quantity of Urine per Minute. | Observations. |
|---|---|---|
| H. M. H. M. | | |
| 9 5 — 9 45 | 0·116 | Doubtful reactions of biliary pigment. |
| 9 45 — 10 50 | 0·105 | |
| Abstraction of 350 cubic centimètres of blood, and injection of 457 cubic centimètres of Distilled Water at 95° F. | | |
| 11 55 — 12 52 | 0·224 | Reaction of biliary pigment, very distinct and beautiful, no albumen. |
| 12 52 — 2 0 | 0·178 | |
| Injection of 1,500 cubic centimètres of Distilled Water at 95° F. into the same vein. | | |
| 3 16 — 4 10 | 0·054 | Red urine, albumen; a few collapsed blood-cells are seen under the microscope. |
| 4 10 — 4 42 | 0·041 | |

The excretion was not arrested this time after the abstraction of the blood. During the injection of the second portion of water the

urine was for a time collected; it contained no albumen. Next morning the urine was again yellow and free from albumen. The reaction of biliary pigment was very distinct.

Now, as to the question, whether after considerable dilution of the blood-plasma, so long as its composition is not essentially altered by the transition of certain matters from the blood-cells, the albumen contained in it passes into the urine, the experiments yield a completely negative result. On the other hand, it appears, contrary to former experiments, that when a certain degree of dilution is attained, albumen, together with colouring matter of the blood, is excreted with the urine, and this period is always attainable with certainty, for we may continue a gradual injection of water until the red-coloured urine becomes visible in the glass tube connected with the canula. The statement that no albumen is excreted unaccompanied with hematosin by the kidneys, after a considerable increase of the water of the blood, is satisfactorily confirmed by a short review of all the experiments. Thus, if we inject water into the veins of an animal, the urine remains clear; the colour does not deviate from the normal condition, and albumen is not demonstrable by the strictest examination. If we add more water, a moment arrives, if the volume corresponding to the size of the animal is employed, when an uniformly red-coloured urine is excreted, in which hematosin and albumen are contained. Until this period arrives, albumen can in no case be observed. After about twelve hours[a] the urine again flows off with its normal colour, and with the hematosin the albumen has disappeared. Now, as we always find albumen combined with colouring matter in the urine, we may assume either that the albumen of the serum transudes from the glomeruli, when the dilution is sufficient, in order to accelerate the current of diffusion between the blood-cells and the plasma, that is, together with hematoglobulin, or that only the albuminous body of the blood-cells, combined with the colouring matter, passes into the tubuli uriniferi. It is, of course, impossible to adduce a rigorous proof of the correctness of one or other assumption, as the few characteristic distinctive signs are lost in the urine. However, reasons are not wanting which render the first assumption very probable. Thus the conditions for the passage of both bodies may occur, but not cease simultaneously, so that the excretion of the albumen of the serum must always outlast that of the hematosin. Besides, the estimation of the amount of iron may with approximative accuracy inform us, whether we have to do with a mixture of both bodies, or with hematoglobulin. If a mixture of both bodies were actually present, it must be attended with a per-centage of iron much less than that resulting from the analyses of C. Schmidt for the blood-cells.

In order to obtain the body pure for some such investigation, a portion of the red-coloured alkaline urine was acidified with acetic acid, and mixed with about twelve times its volume of alcohol.

[a] Probably the excretion of the red-coloured urine ceases earlier; after the termination of the experiment, unfortunately, the discharge could not be so watched as to enable me to determine the period with accuracy.

After the precipitate had duly deposited, it was separated by filtration from the fluid (which was now free from albumen, and had assumed about the ordinary colour of the urine), and was well washed with hot water.

The following composition was deduced from two analyses:—

| | | | |
|---|---|---|---|
| The body dried at 230° F. = 1 | { | Organic matters, . . . | 98·998 |
| | { | Inorganic constituents, | 1·001 |
| | | Whereof iron . . . | 0·491 |
| " = 2 | { | Organic matters, . . . | 98·537 |
| | { | Inorganic constituents, | 1·463 |
| | | Whereof iron . . . | 0·601 |

If we now calculate according to some aualyses of human blood made by C. Schmidt[a], the per-centage of iron of the anhydrous blood-cells, we obtain in 100 parts at 248° F.:—

Non-volatile matters, { 0·313 iron / 0·395

Now this difference, in fact, shows, that the albumen which, on a considerable increase of the water of the blood, passes with the hematosin into the urine, is not a mixture of globulin and the albumen of the serum. But as a separation of the hematosin from the globulin is, under these circumstances, not possible, it follows that the substance in question can be only the modification of albumen belonging to the contents of the blood-cells. It is useless even to raise a suspicion as to the existence of an affection of the kidneys, as the change of the urine did not correspond thereto, and was very transient.

But what relation do the results of these investigations bear to those which establish an excretion of albumen without the colouring matter of the blood? It must be admitted, that on a sudden and considerable dilution of the blood, the albumen of the serum may pass into the urine, but this transudation is not the result of the greater abundance of water, but other conditions are developed by the injection, on which this circumstance depends. The difference in the results depends on the mode of injecting.

Dr. Hoppe had the goodness to call my attention to the sudden and great swelling of the large veins leading to the heart which ensues on the injection of a fluid into any vein, and at the same time to the differences of tension which must arise according to the rapidity of the injection. Hence the obstruction experienced by the current of blood, in consequence of the rapid injection of a large quantity of water, might be so considerable that the pressure of the blood in the part of the circulation concerned should continue sufficient to cause an exudation of albumen, while by a slow and gradual introduction of the stream of water driven from the syringe towards the heart, no obstruction is created, either directly, or by the increase of the fluid of the blood, which is not quickly removed again by the

[a] Characteristik der epidemischen Cholera gegenüber verwandten Transudationsanomalien. 1850.

contractions of the heart. A rapid injection would, therefore, have about the same effect as tying the vena cava or the renal vein.

When Kierulf states that an exudation of albumen ensues in the first instance, the view just now expressed would find support in the difference of the times employed in the injections. Kierulf injected 495 grammes of water in three minutes, 492 grammes in two minutes, 660 grammes in five minutes; in my experiments at least fifteen minutes were employed in injecting 400 grammes. Moreover, his results in general may be the more certainly ascribed to increased pressure of the blood, as it would appear that in the red-coloured urine, with the exception of a single case, a considerable number of blood-cells was always found under the micoscrope. It deserves, too, to be particularly mentioned here, that the transudation of albumen, when attributable to a change in the quality of the blood, that is, to a very dilute condition of it, must last a long time; for a great excess of water, as can be proved by comparative analyses, is still present after many weeks. As a result of increased tension of the blood, it, like the latter, is transient, and as the colour of the urine, at the commencement or end of the excretion of hematosin, may present a very doubtful appearance, a very accurate examination is in such cases highly necessary.

From these investigations the inference would appear to be justified, that the dilution of the blood as such does not effect the transudation of the albumen of the serum into the urine, but that it to a certain extent sets free a considerable quantity of hematoglobulin, for which the vascular walls of the glomeruli are, under conditions unknown to us, more permeable than for albumen.

The last experiment must be particularly mentioned, as in it the microscope revealed a small number of blood-cells; in this case there is no doubt that, notwithstanding the previous abstraction of blood and the slowness of the injection, there was increased pressure of blood, as the quantity of water injected (1975 cubic centimètres), was much too large. I have quite lately learned that Hartner[a], who instituted experiments on rabbits, bearing on the same point, found the urine voided from the bladder always red-coloured, and thence, likewise, drew the conclusion, that it must be the contents of the blood-cells.

Another change produced by the dilution of the blood is the occurrence of biliary pigment. This colouring matter was almost always found before the excretion of hematosin commenced; whether it still continued after the disappearance of the latter from the urine, could not be quite positively ascertained. The time of its occurrence after the injection of the water was not always the same, the reaction at one time appeared very rapidly; at another, somewhat later; the intensity of the reaction was evidently dependent on the degree of the dilution. We should be liable to be deceived did we suppose that we could detect the presence of the pigment with nitric acid containing nitrous acid. In this way the reaction succeeded only

[a] "Bericht über die Fortschritte der Anatomie und Physiologie." 1858. 2 Hft.

once. In most cases, too, the colour of the urine failed to indicate the presence of biliary pigment. It was certainly somewhat darker, but never actually icteric. The presence of biliary pigment is demonstrable, without fail, by pouring, as Kühne directs, nitric acid into a cylinder or test tube, and then cautiously allowing the urine to trickle down the side of the glass, so that the fluids shall not mix with one another; the characteristic colours occur in the layer of contact. The presence of the colouring matter is exhibited in still another mode, by treating the urine with chloroform, according to Benecke's directions. It appears worthy of note, that even where the reaction with the urine was most intense, very little biliphiæn was visible under the microscope.

It would naturally occur to us to seek the source of the biliary pigment in the hematoglobulin which passes through the water into the plasma, and the fact appears to be that, where the dilution of the blood is very considerable, hematoglobulin, and when it is less, biliary pigment escapes from the glomeruli. The origin of biliary pigment from hematosin has, as is well known, been rendered probable under similar circumstances by Kühne; for he has shown that the biliary pigment, occurring in the urine after the injection of colourless bile, cannot be formed from the biliary acids, and he has, on the other hand, prominently demonstrated the solution of the blood-cells. The injection of a solution of hematoglobulin yielded, it is true, a doubtful, but not a negative result, as, after the addition of a very small quantity of glycocholate of soda, the urine afforded a very distinct reaction of biliary pigment, while, injected alone, it had only a slight effect; the author therefore ascribed to the biliary acids an essential influence on the process of metamorphosis itself. The foregoing experiments prove both that the change of hematosin into biliary pigment also takes place in fact without the influence of the biliary acids, as Kühne considered possible, and that the passage of the hematosin or of the biliary pigment into the urine depends on the quantity of water injected, as well as on that of the hematoglobulin set free, while such a dependence was not so evident on the injection of the bile.

It would lead to useless discussion to enter at greater length into such questions as why, after the direct introduction of a solution of hematoglobulin, the metamorphosis is less apparent than after the injection of water combined with much disturbance; why before the attainment of a certain degree of dilution, biliary pigment alone passes into the urine, &c. But it appears to be important to bring into connexion the influences of bile and water on the excretion of the albumen, as by these means, though in a different mode, the same effect is produced, namely, the assumption of the contents of the blood-cells into the plasma. After the injection, too, of the salts of the acids of the bile, the hematoglobulin passes either for the most part unchanged into the urine, while later only biliary pigment with or without traces of the albuminous body is excreted, or, for reasons with which we are unacquainted, hematosin, as such, does not make its appearance at all. The direct introduction of a

solution of hematoglobulin is always followed, as it would appear, by an excretion of the albuminous body. In view of these phenomena, it cannot be disputed that it is possible that after the passage of hematoglobulin into the plasma, in consequence of an increase of the amount of water in the latter, the albuminous body, with or without biliary pigment, might pass into the urine. The nature of the body transuding under the circumstances described must even have a special influence on the bare probability, for it would appear that from the plasma, at the same pressure of filtration, the one albuminous body passes through the vascular membranes, while the other remains behind, and that therefore the serum-albuminate must stand in a quite different relation to its solution from the body of the blood-cells which has accidentally entered. The pathologist would also see that the disturbances of the composition of the blood, proceeding from the relative increase of the water, which, moreover, can never exist in man in so high a degree as is artificially produced in these experiments, can never give rise to a morbid excretion of albumen (albuminuria), while the kidneys continue sound.

According to the experiments hitherto made on the rapidity of excretion of the urine, it has been shown that the quantities of urine excreted by each kidney in the same time—the tension and composition of the blood remaining unchanged—are very unequal, whether compared with one another or with those of the other kidney. If we start from the supposition, that the tension of the arterial blood has not diminished, and that the other unknown conditions for the excretion have remained the same, we should expect that, with the augmentation of the amount of water[a] of the blood, the discharge of the water of the urine should be accelerated, not uniformly, but that it should not sink below an augmented minimum corresponding to the amount of water. If we take away the two experiments in which an excretion of hemato-globulin ensued, and that in which the abstraction of blood effected a diminution of tension, and consider only the others as affording a measure, the effects of dilution of the blood would seem to present but little congruity. The excretion, in the one case, was proportionably increased; in the second, it for a time did not at all increase, and was only slowly augmented; in the third it was even diminished. As, while the composition of the urine is normal, the negative results cannot be referred to an alteration of the renal parenchyma; and as it was only in the first portions of urine excreted after the injection that no increase ensued, the causes must apparently be sought only in a transient influence of the injection on the circulation. Nor can our suspicion rest on the altered quality of the blood, as the rapidity subsequently increases, correspondingly not only in the excretion of a normal urine, but also of one containing the constituents of the blood-cells. The acceleration, when once it occurred, continued, so far as it could be observed, for a long time. Meanwhile, it is quite evident that the fluctuations are much more striking than in the normal state.—*Virchow's Archiv*, 1859, p. 451.

[a] It will be understood, that in these remarks a limit of dilution is supposed.